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The Teaching of the History of Science

(Third Article)

I do not apologize for writing a third article on this subject (1). In all probability it is not the last one. There is no doubt that within the last ten years interest in the history of science has grown tremendously among scientists, historians, philosophers. Educators and college administrators are beginning to take notice of it. But we must not be surprised if this growing interest involves many misunderstandings, for we do not generally understand things until we have misunderstood them in various ways. It is as if we were turning around them and could not really comprehend them until we had seen them from many different angles.

Similar errors and hesitations occurred within the last fifty years with regard to the history of religion and the history of art. These errors will be sufficiently illustrated by recalling a time not yet very distant when a returned missionary was considered almost *ipso facto* a proper candidate for the teaching of the history of religion, and when any scholar having spent a few months in the museums of Europe was readily taken as an authority on the history of art!

Apparently we have now reached that very stage with regard to the history of science, and we need not feel discouraged about it. On the contrary, this proves that a real progress has been made. In the beginning the subject did not exist at all, except in the minds of a few pioneers; now it is generally misunderstood but that implies that its existence is recognized. My aim is

(1) The two other articles bearing the same title were published in the *Scientific Monthly* (New York, Sept. 1918, 193-211) and in *Isis* (vol. 4, 225-49, 1922). Moreover I have discussed the subject in many reviews and prefaces, and in articles ostensibly devoted to other topics. This third article is largely independent of my earlier publications. I wrote it at the sea shore on the basis of a sheet of notes put together for a lecture delivered before the Historical and Medical Departments of the University of Chicago, on Jan. 22, 1929.

to hasten the day when it will be generally recognized and taken for what it really is and not for something else.

So it is necessary to begin with definitions. When we speak of the history of something, the center of our interest is that something. The words « history of » indicate the point of view, the method. For example, the « history of America » and the « geography of America » are two subjects focused upon Americana. They differ in that the Americana are classified chronologically in the first case and spatially in the second. They represent two ways of looking at America : how did it grow ?, how was it built ?

Similarly the history of science is at once history and science, but it is primarily science. It is the consideration of scientific facts and ideas, not in their own systematic and logical order, but in their chronological order. It is the answer to the question : how did our scientific knowledge grow ? Trough which stages did it pass before attaining the present one ? History is the method, the way of looking at. Science is the kernel. Our interest in the history of science is a function of our knowledge and of our understanding of science. To be sure it also depends upon the earnestness of our historical bent, but whatever curiosity we may have of the past, we shall apply it preferably to the objects which we know and which we love. Even as the history of the United States is more interesting to an American than to a Spaniard, the history of science is more interesting to a scientist than to an artist.

The better we know and love a thing, the more information we crave about it, not simply as it is now, but as it was before. We love its past because of its present. This is the natural way, but in some cases the order may be reversed. A true historian will always feel that he does not know a thing and that he does not love it well enough until he has traced it back to its origins. He is so accustomed to consider things not as they are but as they become that he can no longer see them statically; he must see them grow. In such cases, history becomes a means of knowledge, a love spring. In reality the two processes succeed and complete one another and are but two aspects of the great rhythm of life. We can know only the things which we love, and we can love only those which we know.

To return to our definitions. The history of science, i.e., the classification and understanding of all knowledge in chronological sequence, is of course an immense subject. Few people are able to embrace all knowledge, be it only from a narrow angle. Happily it is not more necessary for this subject than for any other that every scholar should attain a complete mastery of it. No subject is too vast, even for the smallest minds, if one need only consider a part of it at a time. The history of science subdivides itself naturally in two main ways—(A) scientifically, and (B) historically.

(A). The historian is not obliged to study the whole tree of knowledge. He may restrict his attention to one of the main branches, say, mathematics or medicine, or to one of the secondary branches, the theory of numbers or ophthalmology, or even to a single twig, FERMAT's theorem or the cataract.

I am especially interested in the history of science as a whole, in the history of the whole tree, and it should be observed at once, that the history of science is a vaster subject than the sum of all the special histories. A full tree is essentially different from, and superior to, a collection of twigs or branches. It is these twigs and branches plus something different and greater, its very treeness which is lost in the cutting. Indeed the most interesting part of the history of science is the study of the communications and interactions between the branches, the correlativeness and unity of knowledge. My comparison of knowledge with a tree is suggestive but inadequate, for the links between different parts (or « branches ») of knowledge, are far more varied and intricate than the simple and regular subdivision of branches into smaller ones.

The history of any special branch of knowledge compensates its inferiority with regard to the larger subject, by a greater technical richness. To put it briefly, what it loses in philosophical value it regains in technical interest; it is less educative but more instructive.

(B). The historical subdivision hardly requires explanation. One may wish to study the complete evolution from prehistoric times to our own; or else one may confine oneself within a special period, as large or small as one cares for, or restrict one's endeavor to the consideration of a special race or group. Such subdivision

is especially necessary for the better understanding of periods or groups the investigation of which calls for a special technique. The student of primitive science must be thoroughly trained as an ethnologist; the student of Chinese science must be a Sinologist; and so forth. In many cases the limitations of one's efforts are imposed by purely linguistic difficulties. It is obvious that an original study of ancient science demands a practical knowledge of Greek and Latin; it is less obvious but equally true that a complete understanding of mediaeval science requires as well some knowledge of Arabic and even of Hebrew.

It is foolish to say that this or that period was more important than the others. None was absolutely superior to the others, not even the Periclean age; most had their own special superiority; all were necessary, none self-sufficient or final. Each period was the result of anterior efforts, and the womb of the following ones. Each period is in many ways the summary and the synthesis of all the preceding ones, and this alone justifies historical research. The past is not past; it is an essential part of the present and the future and continues to live in them.

However one great distinction must be made, for practical and pedagogical reasons, between modern science on the one hand, and the rest of the past, on the other. When did modern science begin is, of course, impossible to say. The answer would vary for each science, and probably for each scientist, and would be tentative in every case. But we may say roughly that modern science is that of the end of the nineteenth century and of the twentieth, and more vaguely still, we may mean it to include all the later developments which have become too technical to be explained in a general course.

Though each historical lecture should be enlightened by references to the latest scientific conceptions, the professor teaching the history of science should hardly be expected to deal with modern science otherwise than by sampling it according to his own scientific training and to the possibilities of his audience. The teaching of the latest discoveries is really outside of his province and may be safely left to the professors in charge of the special scientific courses. Each of these, if he be not utterly devoid of historical sense, will naturally tell the latest history,—the yesterday—of his own subject.

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To complete this defintion of the history of science it is useful to differentiate it from other subjects with which it is often confused or with which it risks being entangled. But before doing this, I hasten to add that such confusions and entanglements are not necessarily harmful. It is well that any subject should be studied from as many angles as possible and by scholars trained in different ways. Each time that this is done—assuming that it is done honestly—the subject is enriched. Some of the best work on mediaeval science we owe to scholars who had no scientific training. Some of the best work in every line was done by *amateurs*,—and the very pioneers are always self-taught. The fact remains that a good preliminary training saves time, energy, and reduces the causes of error considerably. The amateurs and autodidacts have their day, but sooner or later they must give way to professionals, or else progress is uncertain and our very attainments are jeopardized.

The economic value of science has been stressed so often, « Knowledge is power », « it pays to know », etc., that there has been a tendency among economic historians, at any rate among the most advanced of them, to annex the history of science and consider it a part, even an integral part, of their own discipline. This may broaden economic history, but it mutilates the history of science, for the economic aspect, important as it is, is but one aspect of it, and not by any means the highest. The fact that new inventions change radically the human « milieu », that they may at almost any time transform a given community into a different one, is so obvious that the historian of science may take it for granted and save his energy for the exhibition and discussion of other aspects less understood. It is part of his duty to show that scientific studies would still be of immense signification even if their economic value were inferior to that of Greek studies. The main signification of science lies in the fact that it alone can explain our relations to the universe, and help us to comprehend its unity and to contemplate its full glory. The development of science is primarily due to man's instinctive craving for truth and understanding. The intellectual value of truth is independent of its commercial value, and however great

the latter, it is incomparably greater. On the other hand all that we know, all that we do, all that we have, all that we are, is based on truth. That is, upon what we believe to be consistent with reality. As soon as we suspect that our basis is not true, or not entirely so, we become anxious and can have no peace until the matter has been proved to the bottom, and the error removed, or the lower truth replaced by a higher one. Truth is the rock upon which we stand. Take it away and everything crumbles to pieces.

It is in this sense that we can say that the history of science is the very center of the history of civilization. It explains the progress of man's industry and of his power over the forces of nature, but what matters even more, it accounts for the development of his own consciousness. I like to think of it as an unfolding, a true « evolution »,—nature being gradually unveiled to the eyes of man, and his own relative greatness and mediocrity becoming clearer at every step. What is really gained? asks the philosopher. Does the scientist know his purpose better than the Hebrew prophets? Will he ever know his purpose otherwise than by obeying the deepest instincts of his soul, his love of truth and beauty, his charity? At any rate he knows infinitely better his relationship to the rest of the universe. He does not believe any more that he is the center of it, but he knows that, for good or evil, he is an essential part of it, a part of its mind.

Moreover this is not a matter of knowing our purpose or not. Human history as opposed to natural history or « physics » is the history of man, and thus it must be focused upon the development of man, and if anything has developed it is his mind, perhaps not its very substance, but its methods and contents.

The economic point of view leads one to attach undue importance to the applications of science; the history of science is narrowed down to the history of technology. To preserve its full cultural value the history of science must give a place to the applications, but that place must be a subordinate one.

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Another mistake which is often made is to confuse our subject with the history of scientific method. Of course this is a part

of the history of science, but the logicians who are especially interested in scientific method are likely to dispose of historical questions in too summary a fashion, and to devote the whole of their energy to the discussion of this or that method. This is less history than an exploitation of it for a different, philosophical, purpose. Needless to say the history of scientific method is extremely useful, to such an extent that it does not matter so much if it is made the vehicle of a number of historical errors,—as long as it is not taken too seriously from the historical point of view. Unfortunately, for many philosophers, that is the history of science, the whole of it; some very distinguished ones have used the history of science as a quarry whence to extract material for their own constructions. This is legitimate, but premature. Let us first study the history of science, and afterwards apply our knowledge of it to other purposes.

I believe I have now indicated the main dangers which beset the historian of science, the philosophic danger or premature abstraction, and the utilitarian danger or the distortion caused by paying too much attention to the applications of science, too little to the principles. These two dangers, it may be noticed, represent two opposite extremes. The historian of science must learn to steer his course between them, bearing always in mind that pure science is as distinct from philosophical speculation as it is from technical invention.

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Let us now consider the qualifications of teachers. Scholars to whom a course on the history of science is intrusted should be primarily scientists, having an intimate knowledge of the experimental method. Generally speaking a mathematician would not be properly qualified; of course he might be admirably qualified to teach the history of mathematics, but *not* to teach the history of science, because his knowledge of the experimental method is likely to be inexistent, or purely theoretical like that of a philosopher. It does not matter in which special field the experimental technique has been acquired but the genuineness of that technique matters very much. For example, the carefully prepared experiments which are made by students in laboratory

courses are not in themselves sufficient; it is necessary to have made at least some experiments into the unknown.

While any sound scientific preparation would be good enough to form the teacher's scientific background, a physiological one would perhaps be the best, as it would imply a preliminary training in a great variety of subjects, and some familiarity with the ever recurring conflict between the mechanical and biological points of view.

At any rate the teacher must be primarily a scientist, for a course on the history of science would be an evil thing indeed if it became the vehicle of false or inaccurate scientific ideas. But he must be also, if secondarily, a historian and philosopher. Of course his very interest in the history of science suggests a certain amount of historical curiosity, it proves the assumption of a historical attitude—which is essential—but it is a question whether that attitude is genuine or not and well understood by himself. Such an attitude is very complex for it involves an inclination to penetrate origins and to ravel out chronological sequences, a willingness to replace ideas in their own background before attempting to judge them, the faculty of seeing the past live in the present and the present in the past. Moreover he must have a sense of historical accuracy, causing him to suffer from historical errors, at least from avoidable ones, with the same intensity as from scientific or logical ones. Nowhere does the immaturity of our studies appear more clearly than here: scholars who would feel dishonored by the commission of scientific errors which they could have easily avoided, indulge in historical statements with the utmost levity.

The philosophical qualifications need not detain us long, for they are obvious enough. The historian of science must have a good epistemological training. Even if he has no intention of discussing epistemological questions, he must be aware of their presence. He must be able to discuss the logical as well as the chronological sequence of scientific facts, and to examine and compare scientific methods. Above all he must have a sufficient power of generalization to recognize the cardinal facts and the main theories and give them due prominence in his teaching. The relative importance attached to facts and theories by contemporary opinion is often wrong, and it is one of the historian's

duties to correct it in the light of further events. A similar remark applies to men of science. In this field as in any other, the contemporary classification of men is often at fault. Men who were deemed very important seem much smaller, and others who were neglected and despised, much greater, when their lives and works are contemplated from a distance. The correction of such injustice is the historian's privilege, but it puts a heavy responsibility upon him. It is the main test of his historical sense, and also of his common sense, of his wisdom.

Of linguistic qualifications it is hardly necessary to speak. It is almost a truism to state the need of a reading knowledge of German, French, and Italian. This will always be necessary. Other languages may be needed according to the special investigation in hand, the most important being Greek, Latin, Arabic, Hebrew, and the European languages not yet mentioned. At present I am thinking of the teacher, who would already be well off if he knew German and French, not so much of the special investigator whose needs vary considerably and are properly endless.

One of the reasons why he must be primarily a scientist, is that scientific knowledge being essentially systematic, must be acquired in an orderly sequence. It cannot be built up bit by bit, haphazardly, but only in a methodical way, like a monument. This costs time and thought, and it requires careful guidance. It should be noted that a great part of science, notably the descriptive part of it and much of the experimental technique, can be learned to great advantage in early youth. For historical knowledge I believe the opposite to be true. First, the order of study is much more flexible. For example one can thoroughly understand the history of America without knowing that of Egypt. Second, the more mature we are and the better we are able to appreciate it. How could children understand the passions and conflicts of grown men, and yet it is these which form the very texture of history.

It is better to have some experience of life before learning its history; even so, it is better to obtain some intimate knowledge of things and men, and then we shall be naturally interested in the history of that knowledge.

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I have spoken thus far only of the intellectual requirements. For a proper teaching of the history of science a number of material accessories are equally necessary. Such teaching should be given in a hall equipped for the making of simple physical, chemical, and biological demonstrations. This implies the possession of various apparatus, some of which, it is true, might be borrowed from other laboratories as the occasions for them arose. Moreover the teacher ought to have good sets of wall charts, maps, models, etc. Many of these charts and specimens would be of the kind required in the scientific courses, but some at least would be of a purely historical nature. For example to explain the discovery of the circulation of the blood, it would be well to have a chart illustrating the erroneous Galenic views, as well as the usual one. Cheap models of ancient instruments would also be exceedingly useful; e.g., the navigation instruments used by the early navigators, the apparatus of the alchemists, the first microscopes and telescopes, etc. It is clear that such tools would help not only to make matters clearer to the students, but also to fix them in their memory.

However the main advantage of these paraphernalia is that their very presence would oblige the teacher to eschew vague and worthless generalities, to stick to scientific and historical realities, and to introduce the handicraft part of science which is perhaps the most fertile. For though science is a creation of the human mind, the mind could never have developed it unaided by material devices. Libraries and museums are like extensions of our memories, and even, as far as they are classified, of other parts of our intelligence. But I was not thinking of them, rather of the instruments used by natural philosophers. Much of our experimental knowledge—and with few exceptions the best of it—developed almost naturally together with the instruments which human hands had built and used. It is truly essential to exhibit this interdependence of intellectual processes and handicraft, of minds and tools. This is the only way that one can realize the very life of science, what one might call its own reproduction power. For the minds of men are inspired and guided by the instruments which other minds have invented,

and each instrument is like a center of intellectual crystallization. And so on: the younger scientists improve the instruments bequeathed to them, and better instruments make better scientists.

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A general course on the history of science could be organized in many different ways. I shall give my own conception of it for the sake of example. I would divide such a course into four or five independent semestrial courses of thirty to forty lectures each, as follows:

(1) Antiquity
(2) Middle Ages
(3) Sixteenth and seventeenth centuries
(4) Eighteenth century
(5) Nineteenth century, with references to the twentieth.

The two last ones might be united into one. The whole course would thus cover four or five semesters.

The four (or five) parts of the course should be independent to enable students of different kinds to follow only one or two of them if they chose. Only students planning to make a special study of the history of science, and possibly to devote their life to it, would be expected to take the complete course, and in addition they would have to attend seminary exercises and to carry on investigations under the instructor's guidance. It would be better for them to take this course in its chronological sequence but if it so happened that they entered their junior year at the time when the instructor had reached the third or fourth part, they might begin then and study parts one and two later. This would not be as harmful as it might seem. Even so a scholar might study profitably American history first, and Greek history later, though the opposite order would be preferable.

Classical students would be advised to take the first course, and it would be indeed very helpful to them. It is unpleasant to come across some of them—not only students but teachers—whose ideas of ancient science are extremely rudimentary. Their ignorance of the subject is sometimes so deep, that they are not ashamed of it. And yet he who does not know Greek science, has not really penetrated the Greek miracle. The development

of Greek medicine or geometry is as admirable as that of Greek tragedy, philosophy, or sculpture. But it has this additional interest that it is somewhat easier to account for it by connecting it with earlier developments which occurred in other countries. We know no prototypes to the Greek tragedies, and the archaic sculpture of Hellas is so remote from that of Egypt that we are obliged to postulate independent evolutions. But for mathematics and medicine, we can, if not reconstruct, at least evoke two millennia of earlier experience.

In the same way every mediaevalist should be advised to take the second course. In a sense this course would be even more useful to him than the first to the classical scholar. Indeed mediaevalists have given us an entirely distorted view of the Middle Ages, because of their failure to consider the evolution of positive knowledge and of technique, and to take into account the enormous intellectual activity of Islam and Israel, and the valuable contributions of India and China. Most mediaevalists are Latinists, and they have led us to believe that the Latin side of the Middle Ages was, so-to-say, the whole show, when it was in fact relatively insignificant for many centuries. From the eighth to the eleventh century the main intellectual efforts were made under the patronage of Islam; in the twelfth century considerable efforts were still made by Muslims and Jews. After that the importance of Christendom increased by leaps and bounds, but the achievements of individual Muslims and Jews remained very important, and one cannot discard them without seeing the total growth of mankind in a wrong perspective.

Similar remarks might be offered with regard to the other courses. The third might interest students of European history, also those devoting themselves to art and literature, while the fourth and fifth would appeal especially to students of science and philosophy.

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So much for the general course, but such course (or courses) should be completed by a series of special courses devoted to separate sciences. I have already indicated the essential difference between the history of science as a whole on the one hand and the histories of particular branches of science on the other. In the

general courses attended by students whose scientific background is as heterogeneous as possible, it is clear that technicalities would have to be reduced to a minimum. The instructor should relate the history of only such ideas of which he could give a sufficient explanation without digressing too far; this would oblige him to eliminate certain topics, notably mathematical ones. Hence the general course would remain, at its best, somewhat incomplete and vague from the purely scientific point of view. As opposed to it, the histories of special sciences would be far more concrete, more technical, and more rigorous.

On account of that essential difference, the qualifications of instructors would be different too. To put it briefly, those giving the general course should be better historians, those teaching the special histories, better scientists. It would necessarily happen that the instructor in charge of the general course would speak of discoveries which were outside of his immediate experience. On the contrary no one should be allowed to teach the history of mathematics, who was not a trained mathematician, nor the history of medicine, who was not an experienced physician. I am not thinking so much of theoretical knowledge which an intelligent and energetic layman could possibly obtain, but of the practical knowledge which is largely esoteric, and which is in some ways more pregnant, because it is nearer to life. To appreciate the finest points of medical evolution, one must have been brought up in the atmosphere of a medical school, of hospitals, one must have acquired a physician's outlook. It stands to reason that he who would teach the history of a science, should be as familiar with it as possible. If not, his course—even if technically correct—will be full of little misconceptions and will finally mislead students, instead of guiding and inspiring them.

It would be difficult to say which is most necessary—the general course or any of the special ones—because they answer such different needs. But the usefulness of the special courses, great as it is, is restricted for each of them to a much smaller group of students. It is, I believe, essential—for educational and even for technical reasons—that a physicist should have some knowledge of the history of physics, but such knowledge is less necessary to other students, except those planning to become historians of science.

By the way, the necessity of such special courses was clearly realized years ago by the Belgian government, which prescribed that nobody could obtain a doctor's degree in any science without having satisfied the examinators that he had some knowledge of its history. Unfortunately this remained hardly more than a pious wish because the teaching of that history instead of being intrusted to specialists was abandoned to the mercy of scientists, few of whom were sufficiently interested to make a genuine attempt to master it. One must bear in mind that the average scientist is generally too much engrossed in his own problems to pay much attention to history, even to the history of his own discipline. Most of the Belgian courses, I am afraid, were superficial and amateurish, yet even that was better than nothing.

The principle was undoubtedly excellent. No one should be recognized a master in any subject who did not know at least the outline of its history. Of course it would be foolish to expect him to have any deep historical knowledge, but he should know the main landmarks and the leading personalities,—he should be acquainted with his scientific ancestors.

This is almost a moral obligation. We might compare it to the obligation for any educated citizen to know the history of his country. The obligation is of the same kind and of the same order of magnitude in both cases. That is, we don't expect the average American gentleman to be highly versed in American history, but we would be shocked if he betrayed ignorance of the great events. We have a right to expect every scientist to have a similar knowledge with regard to the very science to which his life is devoted, and which constitutes so-to-say his intellectual fatherland. For a physicist, not to be sufficiently familiar with GALILEO and NEWTON, is just as shocking as for an American not to know WASHINGTON and LINCOLN.

It would not be practical, nor would it be necessary, to organize such courses for every science, but they should be provided for every main group. For example, in a large university eight courses should be offered dealing respectively with the following sciences (or groups of sciences):

(1) Mathematics
(2) Astronomy
(3) Physics

(4) Chemistry
(5) Biology (including Psychology)
(6) Geography and Geology
(7) Anthropology, ethnology, sociology
(8) Medicine.

The subdivision of these courses, and their very titles, might vary somewhat according to local circumstances or to the personalities of the teachers. For example, psychology might be dealt with in the history of biology or in that of medicine (together with anatomy and physiology), or in a separate course, or still it might be included in the history of philosophy. I suppose this last solution has often been chosen, and by the way, is it not remarkable that the history of philosophy has been taught elaborately for ages, and that the history of science must still fight for recognition?

While it would be almost a crime to intrust a general course on the history of science to a professor lacking a thorough historical training, these special courses might be committed, at least in the beginning, to scientists having a modicum of historical curiosity.

It should be noted that one or two of these special courses might be taken care of by the instructor in charge of the general course in accordance with his own scientific training. For example, I have given on various occasions, the first two courses (History of mathematics; History of physics), and if I had time, I would gladly undertake to teach the history of astronomy or chemistry, but I would not like to teach the history of biology or geology or medicine (though I deal with such subjects in my general course) because my own experience in these fields was not sufficiently direct nor sufficiently long.

A total staff of three to five professors (i.e., instructors of different grades) would be needed to give the five general courses and the eight special ones (i.e., thirteen semestrial courses), plus some more advanced ones, and to conduct the seminaries completing them. This would involve a not inconsiderable expenditure, but some of it could possibly be charged against the special departments concerned. For example, the cost of a course on the history of physics would be almost insignificant as compared with the total maintenance of the department of physics. Moreover as I shall try to show presently, the organization of these historical

courses would transform and purify the intellectual atmosphere of the scientific, historical, and philosophical departments, and would bring them closer together.

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The main purpose of these courses would be best attained if they were organized in a special department, which would be so-to-say a « liaison » department between all the others. One of the chief defects of the present system of college education is its fragmentariness. This seems an unavoidable consequence of scientific progress, and of course the fragmentation which we are now witnessing is only a beginning. Maybe that a hundred years from now pulverization will be a better word for it. At all events, the situation is bad enough as it is. A student taking a number of courses, say, English, Greek, Mathematics, Chemistry, can hardly imagine that these subjects are related. To him they are utterly disconnected, nor are his teachers more comfortable about it, for they speak different languages, think in different ways. The Greek professor knows no chemistry and is rather proud of it. The mathematician has forgotten what little Greek he ever knew and is not aware of any loss. The chemist has long since reached the conclusion that historians and philosophers are windbags and nothing more. How could they understand one another? And if one of their students has an especial longing for intellectual unity and suffers from this disintegration and this kind of drifting, how can they, who hardly appreciate what their colleagues do, allay his anxieties?

And yet all the subjects which are taught in our colleges are deeply united,—branches of the same tree, twigs of the same branches, leaves of the same twigs. To realize it, it almost suffices to think of the past, to replace oneself in an earlier stage when knowledge was less differentiated. The twigs may not know one another but they belong to the same tree, as you can easily see if you go down from twig to branch, and from smaller branches to bigger ones. The Greek miracle was named HIPPOCRATES or ARCHIMEDES, as well as AESCHYLOS or PHIDIAS. It is really the Greek spirit, the spirit of ARISTOTLE, and also to a smaller extent that of PLATO, that we are fighting for. A chemist, who

understood this thoroughly, would be more of a humanist than the Greek professor if the latter did not (which is but too often the case).

The department of the history of science would keep in touch with all the other departments; it would be its very mission to do so. Parts 1, 2, 3 of the general course would put it in communication with the classical and historical departments; parts 4 and 5, with the scientific departments. However the main bridges to the latter would be the special courses. For example, the one on the history of mathematics would be organized with the intimate collaboration of the mathematical department. It would even be an essential feature of the latter, for whatever his special interest, every mathematical student would be advised to take the historical course in his junior or senior year.

The grouping together of all these scientifico-historical courses would be preferable for other reasons: it would facilitate their coordination, their proper distribution between a few specialists, and would make the team work of the latter more effective and more pleasant.

Above all, the purpose of that department being to reestablish synthesis and unity, it is clear that this would be best accomplished if it were not identified with any one of the departments to be reunited. For example, if these courses were organized by the philosophical department—as might well be done—there would be a double danger, first that the teaching would be too philosophical, would become in fact something different (as explained above), second, that the confidence of the historical and chiefly of the scientific departments would not be obtained as easily. Scientists have not forgotten that it took them at least a thousand years to fight the hegemony of theologians and philosophers, and that their own immensely successful career did not really begin until that hegemony was broken. On the other hand if these courses were offered by the historical department it might happen that the philosophic or even the scientific interests—which are predominant—were neglected, or that instructors were appointed whose scientific qualifications were insufficient, and the scientists would drift away from them instead of coming nearer.

There is still this to be considered. Sooner or later it will be necessary to make possible the obtention of master's or doctor's

degrees in this new field, the history of science. This necessity has already been foreseen and satisfied by the University of London. A remarkable program ad hoc was prepared by a committee of London professors under the chairmanship of ALFRED NORTH WHITEHEAD, now of Harvard (see *Isis*, 6, 406-7, 534-37, 1924). If degrees on the history of science could be granted only by either the historical or the philosophical department, it is probable that the requirements would be too severe with regard to the historical and philosophical knowledge, and too indulgent with regard to the scientific knowledge—which is truly essential—and to the history of science proper.

The main requirements of such degrees would be:

(1) Sound knowledge of one branch of science (including experimental work) and of its history.

(2) Lesser knowledge of two other branches and of their history; one of them being sufficiently distant from the main scientific subject. For example, if the main subject were physics, the two others might be astronomy and general biology.

(3) Linguistic abilities to be determined in accordance with the candidate's purpose. A reading knowledge of French and German would be required in every case.

(4) Knowledge of historical method and of general history.

(5) Deeper knowledge of a certain historical period, or of the history of a certain race or people.

(6) Epistemology and logic.

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Whether such a department be organized or not, the most urgent need is the creation of a new group of specialists,—historians of science. Without them the progress of our studies will remain accidental, sporadic, and precarious. Nothing could be more foolish than the policy of college administrators to ask professors of history or of science to extemporize courses on the history of science. I do not deny that such courses may be occasionally good, but the chances are that they are not. Historians, philosophers, scientists are too busy in their own fields; they can hardly be expected to assume the burden of doing pioneer work in another. For to teach the history of science is doing

pioneer work, though some of the men who do it in their own fashion—using uncritically the first books falling into their hands, and blessedly unconscious of the difficulties involved,—may not be aware of it. A dean who would ask a professor of chemistry to prepare a course on American history would make a fool of himself (or rather a pair of fools of them both), and yet how much easier this would be than to prepare a course on the history of science ! I do not mean to imply that American history does not deserve to occupy the whole of a scholar's attention—far from it—yet if I had to teach American history next year I am sure that I could offer a course which would be essentially correct if not original. On the contrary, though I have been teaching the history of science for fourteen years, I have seldom given a lecture which satisfied me entirely. There were in every case too many details which I did not know well enough, and which could not be known without investigations. These investigations had not yet been made and it was impossible to undertake them for the sake of a lecture.

The study of the history of science was organized seriously by the Carnegie Institution of Washington in 1918. Then for the first time a scholar was appointed who could devote to it all of his time and energy. However, this is insufficient, for the activity of a research worker does hardly influence the teaching which has remained exceedingly amateurish to this day. This teaching should be undertaken in earnest by at least one institution. For example one of our leading universities should create a department along the lines indicated above. However small, this department should be independent and intrusted to specialists. If it were abandoned to the mercy of this or that professor, to gratify the latter's historical dilettantism, more harm would be done than good. But if it were really an honest creation, it would soon exert a deep influence not only upon that university but also upon many others. Indeed it would become the nursery for the study and the teaching of the subject in other places. No gift of prophecy is needed to say that a time will come when even the smaller colleges will be deemed incompletely staffed if they can not boast at least one historian of science, acting as a « liaison officer » and as an exponent of the twentieth century humanism.

It may be observed that the history of science has this in common with other historical branches, that while it requires highly trained specialists to promote our knowledge of it, every educated man can enjoy its fruits. In other words, the courses on the history of science are difficult to give, but easy to receive. To give them truly well, no amount of knowledge would suffice, wisdom and intuition would be equally necessary. But if they were truly well given, not only would they appeal to all intelligent students, but they would help them to understand better the rest of their curriculum, and to make a wiser use of it. It would at once humanize and integrate their knowledge.

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There is one curious misunderstanding which I have not yet touched, that is, the confusion between the ideas of « history » and « introduction ». This is due to the fact that one of the best ways of introducing a scientific subject is to explain its birth and early development. This is so true that elementary textbooks on, say, astronomy have sometimes been called « history (or the story) of astronomy ». (This confusion is readily understandable if one realizes the many ambiguities of our word history).

However helpful the historical point of view may be to introduce a scientific subject—to make people understand, for example, what astronomy is and what astronomers do—it does not help us very long. If one were to study a branch of science in the historical order, one would never get to the end of it, and, what is worse, one's knowledge would be badly knit together, and of a very precarious nature. The almost infinite complexity of science would soon bewilder the strongest minds, if it were not steadily counteracted by the synthetic efforts of some scientists. The result of these efforts is to reclassify the items of knowledge in an order which is generally very different from the chronological order, or from the one thought to be the most natural; indeed it may seem extremely artificial. It is often found that the best way of teaching a subject to adult minds is to begin by explaining some of the latest and most abstract notions and using these as the « elements ». Thus the final order of the subject taught is almost the opposite of the chronological order of invention. For

example, a professor of chemistry would (or might) find it convenient to explain at the very beginning—as if these were the most obvious matters—our highly complicated views on the structure of atoms and spectra and the principles of thermodynamics.

Under the influence of that confusion between « history » and « introduction », when courses on the history of science have been improvised in some of our colleges, they have generally been offered to the youngest students, the idea being that such a course would be for them a general initiation and would help them to determine their curriculum.

It is possible that such an « initiation » course is useful—this does not concern us at present—but it is certain that it is very different from the course on the history of science which we have in mind. As explained above, one cannot be genuinely interested in the history of something which one does not know. On the contrary, the more extensive their scientific knowledge, the more would the students appreciate the historical survey which would be offered to them in their junior, or preferably, in their senior year. Far from being introductory, this course should be as far removed as possible toward the end of the curriculum. After having studied many things, each as it were in its own separate box, and in a systematic, logical order, they would review their intellectual acquisitions from the historical point of view. This would gradually bring out the natural but deeper relationships obtaining between objects apparently disconnected; it would help students to weave all their knowledge into a single texture, and incidentally to fix it in their memory. The course on the history of science would not be in any sense introductory, but rather a sort of conclusion, of final coordination. Call it an initiation, if you please—as every general course is—it would not be a scientific but rather the supreme humanistic initiation.

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Our scientific knowledge is increasing continually in every direction and in many different ways. This is especially true of experimental knowledge. Greek grammar and literature will be substantially the same in a thousand years from now, but physics and chemistry will be very different and vastly richer.

Thus the relative importance of science in life and education cannot but increase. This is unavoidable. It is a process of natural growth which is apparently endless, for we cannot conceive the attainment of perfect knowledge. The higher we climb, the wider the horizon, the longer the frontiers of ignorance. The more we know, the more sharply do we realize the limitations of our knowledge.

So on we go! One of the deepest instincts of our nature craves for more knowledge, and we must have it at any cost. To be sure a great many people of the literary or artistic type do not feel this as we do, they crave for beauty rather than for truth, but they cannot stop the growth of knowledge. We are told that some of them are fatuous enough to wish for a stop, for they hate science. But why? We don't hate beauty, we shall never have enough of it. There is not, there cannot be, any conflict between beauty and truth; one may love the one more than the other, but it is sheer perversity to hate either.

And to pretend as some other literary people and self-styled « humanists » do that scientific knowledge does not exist, that is, to behave as if it did not exist, to turn their backs deliberately on it, to pretend that the physicists, chemists, engineers have not troubled the peace of an ignorant world, and troubled it for ever, is even more foolish. It reminds one of those fabulous ostriches hiding their head in the sand not to be seen. The chemists and engineers are come and they are increasing in number and importance. It will not do to ignore them. It is wiser to try to understand their point of view, to share their knowledge, and to exchange prejudices with them, if only for a moment.

Scientific knowledge is a pure blessing but its applications can easily be carried too far. Technicians have often overreached mankind. Most men use tools which they have in no way deserved, and in many cases it would be safer if these tools were out of their hands. Yet is it fair to blame the tools instead of the people misusing them? And are the literary people who complain of this machine age always as candid as one would wish? Are they not often the first to kick when their motor or their radio is out of order? There are many countries where mediaeval conditions still obtain, but have you ever heard of a respectable mediaevalist taking refuge in any one of them to evade the

unspeakable evils of science? Of course not, for they need all the conveniences which science alone can provide to curse our civilization more comfortably.

The increase of knowledge is naturally a source of evil, for nothing can grow and remain pure. The evil will not be remedied by stopping knowledge—this could not be done without stopping as well its blessings which are immeasurably greater—but by purifying it. It can be purified in two ways, by integration and by humanization. The integration is largely accomplished by the scientists themselves, that is, by the more philosophically minded of them. It is mainly a sifting of materials, a better arrangement of those which are kept, a constant reclassification and readjustment of knowledge, involving occasionally the discovery of new elements and principles. Thanks to this process of integration or synthesis the ceaseless extension of science is compensated by its deepening, the increasing complexity is neutralized by progressive simplification.

The historian of science is sometimes able to facilitate this integration, but he is mainly responsible for the use of the other method of purification,—the humanization of knowledge. Indeed to humanize knowledge, without in the least cheapening it, it suffices to consider it—as we have long learned to consider every other human activity—from the historical point of view. Without such humanization, however instructive, science is not truly educative. Together with it, it becomes the most inspiring element of our life.

The main trouble with what I would be tempted to call crude science—positive knowledge, pure and simple, without adornment, without any allowance for human weakness—is that it is not lovable. It would be wrong after all to call it crude; it is not science which is crude but we ourselves; in fact that unlovable science is just the opposite of crude, it is too pure for ordinary life. Unless we be PASCALS or SPINOZAS, we cannot really love the things which are absolutely pure and too remote from our own flesh. Now this matters very much. It is not enough that science be right, it must be lovable. GEORGE ELIOT said : « The first condition of human goodness is something to love; the second, something to reverence » (1). Pure science is too abstract,

(1) Scenes of clerical life. Janet's repentance.

too perfect, too frigid to be either loved or revered, and it is partly because of that that it could never become—as a few enthusiasts have dreamed it—a religion. Science can never have the moral and educational influence which it ought to have unless it is made lovable, and this can only be achieved by injecting into it a little of the historical spirit,—of the spirit of humanity. While austere scientists are constantly trying to increase the rigor and abstraction of science, we must on the contrary bring out the human element of it, and this is done very simply by recalling its human origins and vicissitudes. The most abstract ideas were born out of living concreteness. We forget it only because we are so far removed in time and space from the humble cradles of our boldest thoughts, for it took many stages of abstraction to reach the final ones. The pure scientist is interested only in the final abstractions, and he is tempted to obliterate all the rest, even as an architect is anxious to destroy the scaffolding which hides his building. Our purpose is opposite. We want to know by which ways, straight or devious, the results were attained; we want to understand not only the lifeless generalizations, but the living processes which led to them. We cannot dwell happily in an edifice, however beautiful, unless we know when and how it was built, unless we know something of the people who built it. For we are keenly aware that there is far more in it than architect's ideas and lifeless stones; it was built out of flesh and blood, of joy and pain, of life and death.

Scientists have a disagreeable tendency to be too sure of themselves, and sometimes, a little overbearing. This is not true perhaps of the really great ones, who are so conscious of their own imperfection and of the limitations of science that they are more likely to be as humble as can be,—but the majority are not great. Indeed most scientists, like most men, are rather mediocre, and we must take them as they are. Unless they be unreclaimable, the history of science would help to educate them, by baring to them the many incertitudes, the weaknesses, the very humanity of our knowledge. This would not improve their technical qualifications—not a bit—but it would make better men of them, humbler, less dogmatic, and perhaps more charitable. Outside of the love of truth which I take for granted, for without truth everything becomes shadowy and almost inexistent, I would

gladly say with the Ḥassidim that the three cardinal virtues are humility, cheerfulness, and enthusiasm (1). The history of science would help to cultivate these virtues in the minds of scientists, and thus it would humanize them as well as science itself.

* * *

Instead of thinking only of the creative activity of scientists, which is of course the most important but is at best very limited, we may evoke their negative activity which is far more considerable. They are the guardians of truth, the keepers of the lighthouses. Theirs is the endless task of fighting the circumambient darkness, the rampant superstitions besieging us. For it is not enough to eradicate these once, they will come back like the weeds of our gardens, and if the struggle was stopped for a while, intellectual retrogression would at once set in. The endless fight against unreason, against the primeval and eternal darkness is their mission. And there is nothing abstract in that, is there? It is just as human a fight as the struggle against ugliness or injustice. They are fighting not only for abstract ideas, but for the joy and happiness of human beings; they are fighting for humanity even as the artists or the saints.

The history of that endless struggle is a part of the history of science, and no humanism is complete which does not take full account of it. The struggle for light and truth is the struggle for human existence, as opposed to purely animal existence, and for consciousness; it is the very struggle for the humanities. Indeed the historians of science are the standard bearers of humanism. How is it then that the classical humanists refuse to admit it? that they refuse to see what was as clear as daylight to their ancestors, the humanists of the fourth century B. C.? What has caused them to become blind? I don't know. That is their worry anyhow. But sooner or later they will realize that they cannot keep science out of humanism without making fools of themselves, that they must on the contrary consider it as an essential part of it. It may take some educators a thousand years to understand it, but never mind. In the meanwhile I

(1) The connotation of the original Hebrew terms is somewhat different: shiflut, simḥah, hitlahabut.

am quite happy to continue to preach in the wilderness and to be taken for a barbarian into the bargain. « Barbarus hic ego sum quia non intelligor illis »...

Moreover the dawn of a new day is slowly breaking. Little groups are gathering here and there—scientists, historians, philosophers. The time is approaching when it will no longer be possible for the old grammarians to monopolize the duty and glamour of education, and when they will no longer be able to keep aloof from the scientists and vice versa. The time is approaching when the great intellectual division will no longer be scientists and non-scientists, but humanists and non-humanists. And the humanists, the New Humanists, will include many men of science, and the non-humanists will include many of the old grammarians who will persist in considering only one face of life, and clamouring that the others do not count. The humanism of our time will make as large a place for science as for art and literature and religion, or, it will perish.

Nantucket, Massachusetts GEORGE SARTON.
September, 1929.

Richard Kirwan, an Irish chemist of the Eighteenth Century.

RICHARD KIRWAN's contributions to the various branches of science were of the greatest importance, and his researches in chemistry did much to further that science and extend the boundaries of our knowledge. His allegiance to the Phlogistic Hypothesis, in common with several of his celebrated contemporaries, may be attributed to the chaotic state of chemistry at that period.

He was born at Cloughballymore, Co. Galway, in the year 1733. The KIRWANS were descended from an ancient English family who emigrated to Ireland in the reign of HENRY THE SIXTH.

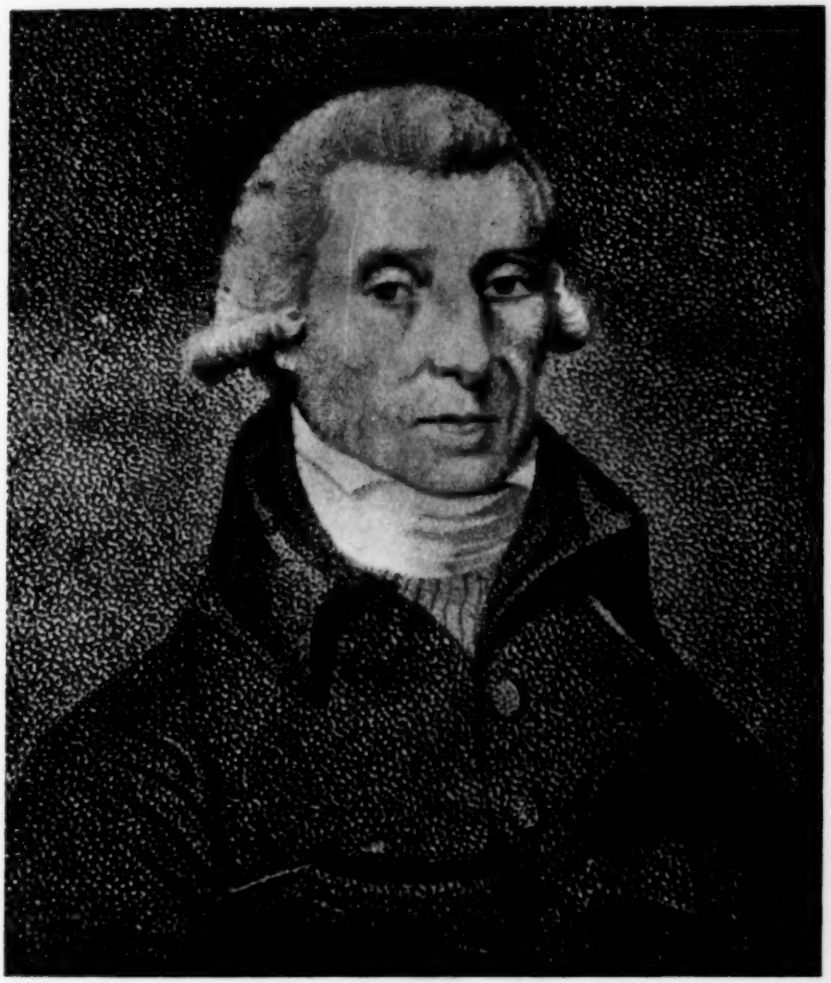
RICHARD was the second son of MARTIN KIRWAN, his elder brother PATRICK being heir to the family fortunes. RICHARD was destined for a profession, and that of a clergyman of the Church of Rome was selected for him.

Even in early youth he displayed unusual intelligence. When only five years old he could conjugate a French verb, and at seven he wrote an abridgement of ancient history.

« Such was his devotion to study at this early period, that to avoid disturbance, he used to ascend a tree with a book, borrowed without leave from his uncle's library, and read for hours regardless of the shouts of servants sent in search of him. He read every morning in bed, from dawn until the family hour of rising, and then concealed his borrowed treasure until next morning. It is not surprising that he should have been intended for a learned profession. »

RICHARD was instructed at Cloughballymore by the Rev. NICHOLAS M'NELLY, a Dominican friar, and at the free school on ERASMUS SMITH's foundation. At the age of seventeen he was sent to Poictiers where he remained for four years.

At Poictiers RICHARD refused to study French, and it was



RICHARD KIRWAN (1733-1812)

Isis, XIII, pl. 3 Portrait by HUGH D. HAMILTON J. REILLY and N. O' FLYNN.

only by an ingenious stratagem his tutor overcame his repugnance. RICHARD spent his leisure reading Chemistry books that he brought with him from Ireland. His tutor removed these, putting in their stead French works on that science. KIRWAN's love for Chemistry conquered and he resigned himself to the study of the French language. About the beginning of the year 1754 RICHARD left Poictiers for Paris. He appears about this time to have entered on his novitiate. In May 1755 we find him at Hesden in the Catholic Netherlands; he is about to abandon his clerical training.

Soon after the death of his brother PATRICK, who was killed in a rencontre with the usher of the Irish House of Commons RICHARD thereby inheriting £3000 to £4000 a year, he returned to Dublin, being then in his twenty second year.

In Feb. 1757 he married the sister of Sir ULICK BLAKE. She died eight years after their marriage, leaving two daughters, MARIA THERESA and ELIZA.

At the residence of his mother-in-law, the Dowager Lady BLAKE, RICHARD collected an excellent library, and fitted up a laboratory. His study of chemistry begun at an early age was here continued, and under such favourable conditions his progress was so rapid that he was soon acquainted with as much chemistry as was then known.

His enthusiasm was quenched by an incident to which he often alluded as the cause of his temporary estrangement from chemical pursuits. Dr. BLACK's discoveries in connection with carbonic acid, and the cause of causticity, commanded universal attention at this time. KIRWAN wrote numerous letters to BLACK making observations on his views, but BLACK did not reply. KIRWAN was so disappointed that he relinquished his chemical inquiries, and did not resume them till he abandoned the profession of the law. He and Dr. BLACK were later the best of friends.

From 1777-1787 KIRWAN resided at No. 11 Newman St. near Oxford St., London, making short visits to Ireland at intervals. He now prosecuted his scientific labours. He regularly attended the meetings of the Royal Society, of which he was elected Fellow on the 24 Feb. 1780. He received in 1782 the Copley medal—the highest gift it has to bestow, for a series of papers on Chemical

Affinity (*Phil. Trans.* 1781-1782) promptly translated into German by CRELL.

In London he became acquainted with many of the most notable people of the time. He was the friend and associate of such men as HENRY CAVENDISH, Dr. PRIESTLEY, Dr. FORDYCE, Sir JOSEPH BANKS, Dr. INGENHOUSZ, Sir GEORGE STAUNTON, HORNE TOOKE, CAVALLO; he carried on a wide correspondence and held receptions which formed a rendezvous for distinguished personages.

He was held in such high esteem that we are informed by Lord CLONCURRY that « even during the hottest period of the war his letters were suffered to pass free from all parts of Europe ».

Owing to delicate health KIRWAN returned to Ireland in 1787, where he resumed his literary and scientific career.

About the year 1782 the society from which the Royal Irish Academy afterwards arose was established. It consisted of an indefinite number of members, most of them belonging to the University, who at weekly meetings read papers in turn. In order to make their labours of advantage to their country, they formed a more extensive plan, and admitted such additional names as would add dignity to their new institution. The first paper was dated 13 June 1785.

KIRWAN on his return in 1787 became a member of the Academy, then in its infancy, and its *Transactions* soon became the depository of his valuable and interesting papers. Coming straight from the centre of the latest discoveries in chemistry, he gave a great impetus to its study in Dublin. Between 1788 and 1809 he contributed 38 memoirs to the *Transactions of the Royal Irish Academy*.

On the death of Lord CHARLEMONT in 1799 he was elected President of the Academy, a post which he held until his death. He died as a result of starving out a cold, on the first of June 1812.

When Lord CASTLEREAGH offered to confer a baronetcy upon him in the hope that his great influence would help in promoting the Union, he declined the dignity.

KIRWAN in common with many other great men was not exempt from the eccentricities that generally accompany genius. As he grew old he developed a number of mannerisms and quaint peculiarities. He had a great abhorrence of flies, and allowed his servants a small premium per dozen for killing them. During

dinner at his own house, it was customary to put the chain on the door so that he might not be disturbed. He always solicited permission to wear his hat, and was allowed to do so even in courts of justice.

« In whatever could promote the branches of knowledge which he cultivated, he was liberal, annually allocating a large sum for the support of his well-appointed laboratory, and the supply of his library, into which he admitted all the foreign and domestic journals. The R. I. Academy received a token of his regard in the bequest of the philosophical part of his library. »

KIRWAN's earliest experiments were upon Specific Gravity and Chemical Affinity. It was impossible that the part of chemistry which dealt with the phenomena of affinity could advance much in the phlogiston period, since the proportions by weight were hardly thought of at all. « But the purely qualitative investigation » as has been noted « of a large number of reactions, from whose outcome conclusions were to be drawn regarding the interaction —of individual components, had the effect of maturing good fruit, so that the restless endeavours of chemists to enlighten themselves upon such questions turned out by no means useless. »

It is to KIRWAN that the science is beholden for extensively useful determinations of the force of attractions, denoted numerically as BERGMAN required. His researches were the subject of three papers published in the *Philosophical Trans.* in the years 1781-1782. In these papers he states that he found the quantity of affinity between the three common mineral acids, called menstrua, and six bases, viz. the three alkalies, and the three then known absorbent earths, to be directly as the quantity of these bases required to saturate a given quantity of the menstrua, or inversely, as the quantity of these menstrua required to saturate a given quantity of the bases. Hence by finding the exact quantity of real acid (that is acid freed from water to a certain degree) which is required to saturate a certain quantity of pure base, he obtained at the same time the supposed numerical expression of the force of affinity between the base and the acid menstrua. By proceeding in a similar way, he attempted to find the precise force of affinity between most of the metallic oxides and the real acids. Metals, in order to combine with acids, must, by the Phlogistic Theory, part with phlogiston

in different proportions, according to the acid in which they are dissolved, and the supposed quantity of phlogiston so separated was calculated. The affinity of the three mineral acids and phlogiston is stated numerically in tables included in the paper. Later experiments showed that the quantities of bases which unite to acids, as stated in KIRWAN's first tables, are not, in some respects, accurate; consequently the quantities of affinity, denoted by the quantities of bases and acids, are inaccurate, as appears from his paper, « On the Strength of Acids, and the Proportion of Ingredients in Neutral Salts », contained in the fourth volume of the *Trans. of the R. I. Academy* in 1791.

« By viewing » says KIRWAN, « the quantity of each species of real acid taken up by any particular basis in the first table, it is easy to perceive that this quantity does not exactly follow the hitherto presumed order of affinities between that basis and the different acids, as I have stated it to do in a prior dissertation. My former opinion was grounded, it is true, on experiments, but those experiments themselves were accompanied with inaccuracies unsuspected not only by myself but by all other experimenters, and moreover, combined with a hypothesis relative to the quantity of real acid, which I have since found to be fallacious. »

The above paper, of 1791, is the result of clever and systematic work, « to approximate at least, if not to attain the knowledge of the precise force of affinity, on the principle of the exact quantity of menstrua and bases in compound neutral salts. » The paper contains tables of the quantity of standard acid existing in 100 parts of each of the acid liquors of given specific gravities; and also in each of the neutral salts referred to therein.

Later KIRWAN wrote « Additional Observations on the Proportion of Real Acid in the Three Antient Known Mineral Acids, and on the Ingredients in various Neutral Salts and other Compounds. »

KIRWAN having discovered « the mode of expressing the quantity of acid adopted to be very inconvenient, as in some of these neutral salts an acid still stronger than the assumed standard was found to exist. » This paper contains, also, amendments to several of the determinations in the tables of the former papers; and likewise additions from the experiments of other chemists. In the tables we find the proportions of stronthia and baryta, with regard to acids; and of Carbonic acid with regard to bases.

« But if » says Dr. PEARSON, « as Mr. KIRWAN will endeavour after NEWTON to prove, that bases are passive, we must then compare the strength of attraction of the menstruum A for the basis c, with the strength of attraction of the menstruum B for the basis c; and we must compare the strength of attraction of the menstruum A for the basis d, with the strength of attraction of the menstruum B for the basis d. »

Here it may be explained that when the menstrua united to the bases, in those cases, are presented to one another, the attractions which keep the substances of the two compounds united together in their present state, are distinguished by KIRWAN's terms Quiescent Affinities or attractions ; the opposing attractions which tend to decompose them and produce two new compounds, are named Divellent Affinities.

During a period of nearly eighteen years KIRWAN studied the chemistry of « precise quantities » or forces of chemical attraction and he was almost the only successful labourer of his period.

« If his tables do not exhibit numerically, as was once supposed, the precise forces of chemical attraction, they at least accord perhaps, with all the cases of double elective attraction, in which the substances concerned in those attractions have had their powers determined by Mr. KIRWAN. »

« I have long found these tables » says GEORGE PEARSON, « of such utility in both teaching and investigating the composition of substances, as to make me deeply lament that similar determinations had not been obtained of the chemical attractions of a much greater number of substances. Hence I could not omit, in this work, Mr. KIRWAN's tables, which serve to explain double elective attractions in so many instances. »

GUITON DE MORVEAU, when criticising the principal methods which had been proposed for measuring the strengths of affinities considered KIRWAN's method to be on the whole the most satisfactory. One great difficulty attending the application of KIRWAN's method was the fact that the composition of the same neutral salts were represented by different numbers by BERGMAN, WENZEL and KIRWAN.

Referring to that part of chemistry which treats of chemical affinities or elective attractions, LAVOISIER says :—« Messrs. GEOFFROY, GILBERT, BERGMAN, SCHEELE, DE MORVEAU, KIRWAN,

and many others have collected a great number of particular facts which only await the places which should be assigned to them; but the principal data are lacking, or at least those we have are not sufficiently exact nor sufficiently certain to become the fundamental basis upon which can rest so important a part of chemistry. »

It is as one of the firmest adherents and most eloquent supporters of the Phlogistic Hypothesis that KIRWAN is generally regarded.

Though the Phlogiston theory originated in Germany, and though the most outstanding of the German chemists supported it, adherence to the theory was by no means confined to the chemists of Germany. The most prominent chemists of all nationalities were phlogistonists, for example, in France, MACQUER, DE MORVEAU, in Sweden, BERGMAN, SCHEELE, in Great Britain, BLACK, CAVENDISH, PRIESTLEY, and KIRWAN; although BLACK and KIRWAN indeed ultimately admitted the strength of LAVOISIERS reasoning.

To the phlogistonists, sulphuric acid was the simple body, sulphurous its first compound with phlogiston, sulphur its most complex compound, having in it the most phlogiston. Similarly when in 1774 SCHEELE discovered chlorine by oxidising hydrochloric acid with manganese dioxide, he thought he was causing phlogiston to leave the marine acid, to combine with the manganese so leaving « dephlogisticated marine acid ». As may be seen from this example the function ascribed to phlogiston was not far from the part which is in fact played by hydrogen; just as for us, the term « reduction » can imply a removal of oxygen or an addition of hydrogen. And this identification of phlogiston with hydrogen had, in fact, already been made. KIRWAN especially was one of those who believed phlogiston to be identical with « inflammable air ».

« Whatever difficulties » says a contemporary chemist, « this theory might present it could not be expected that the disciples of these justly celebrated men (STAHL, MACQUER, BERGMAN, SCHEELE) would abandon it without resistance. They accordingly employed, at first, every exertion of their abilities in palliating the contradictions; afterwards they insisted on all the experiments which seemed to favour it, and lastly some among them, while they retained the word phlogiston, concluded by giving it another signification. This is particularly the case with Mr. KIRWAN.

Among the philosophers who have not yet adopted the new doctrine, he is certainly one of those who is the most capable of producing uncertainty in the minds of such persons as decide by authority. His acquaintance with every part of natural philosophy, the discoveries with which he has enriched the sciences, and even the ingenious modifications he has introduced into the theory of phlogiston; all contribute to give weight to his opinions. »

In 1787 KIRWAN wrote his celebrated *Essay on Phlogiston*. « The controversy » says KIRWAN, « is therefore at present confined to a few points, namely, whether the *Inflammable Principle* be found in what are called phlogisticated acids, vegetable acids, fixed air, sulphur, phosphorus, charcoal, and metals. Limited as this controversy appears to be to a small number of bodies, it is nevertheless of great importance, if an exact arrangement of our ideas, and a distinct and true view of the operations of nature be of any importance. The bodies above mentioned are the subject of many, and the instruments of almost all chemical operations; without a knowledge of their composition, and a clear perception of their mode of action, it will be impossible to form even an approximation to a solid theory of this science, the daily accumulation of facts will only increase perplexity and confusion, and if any useful discovery be made, it will be the mere result of chance. »

KIRWAN's book on phlogiston was divided into thirteen sections :

Sect. 1. « Of the Weights of Different Sorts of Air.
Sect. 2. Of the Composition of Acids and General Principles of the New Theory.
Sect. 3. Of the Vitriolic Acid.
Sect. 4. Of the Nitrous (nitric) acid.
Sect. 5. Of the Marine Acid.
Sect. 6. Of Aqua Regia.
Sect. 7. Of the Phosphoric Acid.
Sect. 8. Of the Saccharic Acid.
Sect. 9. Of the Calcination and Reduction of Metals and the Formation of Fixed Air.
Sect. 10. Of the Disolution of Metals.
Sect. 11. Of the Precipitation of Metals.
Sect. 12. Of the Properties of iron in its different states, and its Conversion into Steel.

Sect. 13. Conclusion.

« All acids » says KIRWAN, « consist of two principles, one peculiar to each, which in the opinion of the antiphlogistonists has not as yet been decomposed, and consequently must be looked upon, relatively to the present state of our knowledge, as a simple substance, and the other, pure air in a concrete state, that is deprived of the greater part of its specific heat, and condensed into a smaller volume; the first they call the acid basis, the last the oxegenous principle ». KIRWAN agrees with the antiphlogistonists, that water is a compound of dephlogisticated air (oxygen) and inflammable air (hydrogen) but he thinks that from this combination there does not always result water; that in some circumstances the result is fixed air, or carbonic acid, that in others it is phlogisticated air, or azotic gas, and lastely, that the result is sometimes nitrous gas and nitrous (nitric) acid. All these substances, according to KIRWAN, are composed of the same principles in the same manner, for example, as sulphur and mercury afford either ethiops or cinnabar, according to the heat developed. KIRWAN's ideas on the composition of nitric acid are all founded ultimately upon the supposition that phlogisticated air, or azotic gas, contains phlogiston.

« Mr. KIRWAN thinks with us » says DE FOURCROY, « that the oxalic acid or saccharine does not pre-exist, but is produced by the action of the acid on the nitre. But he differs in the explanation he gives; he thinks that the ethereal oil of the sugar yields its hydrogene or phlogiston to the nitrous bases which become nitrous gas, while the carbonic acid of this acid is fixed in the dephlogisticated oil, and converts it into oxalic acid. »

« KIRWAN » says DE MORVEAU, « departs from this doctrine in three points only.

1. Inflammable gas exists likewise in sulphur, phosphorus, nitrous gas etc, in the state in which it has been called the phlogistic principle.

2. It exists likewise in the metals and in charcoal.

3. By uniting to pure air, it forms fixed air, and it is not till after this composition that the air becomes the acidifying and a constituent part of metallic oxides.

These three propositions, which form the basis of the author's own system, absolutely depend on the proofs of the existence

of phlogiston, or if it be so taken, of inflammable gas in those substances. The author has been well aware of this, and has assumed it in his conclusion, to place in the strongest light such of his proofs as appear to him to be the most conclusive, and thence it is that we shall take them to examine them. »

KIRWAN's modification of the phlogistic theory was favourably received by the most eminent chemists in Europe, and there seemed to have been a general tendency in Britain, Germany and Sweden to adopt it. It occurred to LAVOISIER and his associates, that a satisfactory refutation of this theory, associated as it was with the greatest names in Europe, would be a death-blow to the doctrine of phlogiston, and would give an eclat to the new theory which would render it invincible. Accordingly, Mr. KIRWAN's *Essay on Phlogiston* was translated into French, and the different sections parcelled out among the apostles of the new system, that they might write a refutation of each. LAVOISIER took charge of four sections;—1.2.3.11.; FOURCROY of three;—8.9.10.; BERTHOLLET of three :—4.5.6.; MORVEAU of two :—7.13.; MONGE of one :—12. The book was then published with a refutation at the end of each section. The effect was exactly what might have been anticipated. The experiments of the French chemists appeared so accurate, their arguments so precise, and their refutation so thorough that Mr. KIRWAN himself « with the candour which distinguishes superior minds », acknowledged his defeat and accepted the doctrine of his opponents. Mr. KIRWAN in Jan. 1791, after, as he remarks, « combating in defence of phlogiston for ten years » lays down his arms. « Enfin je mets bas les armes, écrit M. KIRWAN à M. BERTHOLLET, le 26 janvier dernier, et j'abandonne le phlogistique. Je vois clairement qu'il n'y a aucune expérience avérée qui atteste la production de l'air fixe par l'air inflammable pur et, cela étant, il est impossible de soutenir le Système du Phlogistique dans les métaux, le Soufre, etc. — Je donnerai moi même une réfutation de mon essai sur le Phlogistique. »

« His theory » says Dr. DICKSON, « underwent a regular siege from a phalanx of the most formidable antagonists that chemical philosophy ever mustered; their assaults were made with vigour and repelled with dexterity : and it is perhaps not less to the glory of all parties than to the interests of science, that he has

capitulated, and marched out of a fortress no longer tenable, with all the honours of war. »

We find the first scientific treatment of mineralogical chemistry during the phlogiston era. Mineral chemistry had its distinguished exponents in BERGMAN, and a little later in KLAPROTH and VAUQUELIN. Chemical investigation of minerals was carried on, upon the principles which they laid down, by numerous other workers, including LAMPADIUS, BUCHOLZ, WIEGLEB, WESTRUMB, KIRWAN, VALENTIN ROSE the younger, GADOLIN, and EKEBERG. The endeavours made to classify minerals during that period are for the most part characterised by the desire to recognise their chemical as well as physical properties.

KIRWAN was the highest authority on mineralogy in Ireland, and did much to advance the science in that country. Early in 1792 a committee was appointed to treat for the purchase of a celebrated cabinet of mineralogy called the Leskean cabinet, then for sale, and a sum of £1200 was voted for it, but the total cost was practically £1350. On the 8th of November, KIRWAN who had negotiated in the matter, reported that the cabinet was then lodged in the Hawkin's St. house with a collection of shells which he had procured, also an herbarium and a botanical collection. On the Society's behalf, Dr. KIRWAN subjected it to a rigorous examination, when he rectified any errors. This cabinet helped very much in the diffusion of more exact knowledge on the subject of Mineralogy in Ireland, and KIRWAN refers to it in his work on Mineralogy. The collection consisted in all of 7331 specimens, and was considered one of the most perfect monuments of mineralogical ability then existing. It was divided into five parts : —

1. External character of minerals.
2. Classification of minerals.
3. Earth's internal structure.
4. Mineralogical geography.
5. Economical mineralogy.

When Dr. KIRWAN had completed his examination and arrangement of the museum, a medal of Irish gold, with a suitable inscription was presented to him.

« Previous to the year 1780 » says KIRWAN, « Mineralogy

though tolerably understood by many as an art could scarce be deemed a science, being, for want of precise definitions of its objects, incapable of communication; the same substance from some slight variation of appearance, was often denoted by different names, and different substances by the same name ». KIRWAN's *Elements of Mineralogy* was published in 1784. It had much success being the first systematic treatise on the subject in English. It was long regarded as the standard text-book, and was translated into the French, German, and Russian languages. Even now it is an interesting landmark in the history of mineralogy, as it shows the minerals that were known, the system of classification and the amount of knowledge of their composition.

That KIRWAN derived great benefit from his study of the Leskean cabinet is evident from his own words : « From the inspection of this cabinet, the use I was permitted to make of its specimens, and the well-digested catalogue annexed to it, I derived numerous advantages, being enabled to rectify false descriptions, determine the ambiguous, enlarge the defective, discriminate or approximate, as the case might be, substances to which either the same or different denominations had commonly heretofore been erroneously applied and even to add new descriptions, suggest new distinctions, and supply additional characters or tests of substances nearly bordering on each other; for this purpose I scruplously examined the specific gravities of most of the substances mentioned in the following work, as well as their fusibility in various degrees of heat by the help of Mr. WEDGEWOOD's pyrometer; and to extract from these all the light they were capable of affording, I made many new experiments on the fusibility of various combinations of the simple earths, to the advantage as I expect of the arts of pottery and vitrification. »

Referring to the distinctive characteristics of earths and stones he gives the external characters as : colour, shape, lustre, transparency, texture, cohesion, density, adhesion to the tongue or fingers, colour of a streak, diffusion in water, smell and taste. The internal characters are according to KIRWAN : relation to acids, changes operated in low heats, fusibility and other changes in higher degrees of heat, phosphorescence, magnetism, electricity, and lastly the results of a just analysis.

He divided earths and stones into five genera : (1) Calcareous

genus, (2) Barytic genus, (3) Muriatic genus, (4) Argillaceous genus, (5) Siliceous genus. Those are subdivided into species and families, with a detailed account of each variety.

When making a chemical analysis of earths and stones he classified them under seven heads :

1. Those that are wholly or partially soluble and with effervescence in nitrous (nitric) (1) acid, whose specific gravity is 1.4, or higher.

2. Those that are insoluble in nitrous acid 1.4, but wholly or partially soluble, and with effervescence, in nitrous acid 1.25, at 60°.

3. Those that are insoluble in spirit of nitre, or nitrous acid 1.25, but totally or partially soluble and with effervescence, in spirit of nitre 1.10.

4. Those that are soluble in spirit of nitre, in the temperature of the atmosphere, but without effervescence.

5. Those that are soluble in the nitrous acid without effervescence but only in the temperature of from 150° to 180°, or higher.

6. Those that do not effervesce with the nitrous' but effervesce slightly with the concentrated vitriolic acid.

7. Those that are insoluble both in nitrous and vitriolic acid, or only very slightly soluble, and with little or no effervescence, without particular management.

In volume II of his *Elements of Mineralogy* KIRWAN treats of « Salts, Inflammables, and Metallic substances. » « By inflammable substances » says KIRWAN, « I understand all those of mineral origin; whose principal character is inflammability. A criterion which excludes diamonds and metallic substances, though also susceptible of combustion. » These he reduced to six genera, namely : the aeriform, the bituminous, carbonaceous, vegeto carbonaceous, vegeto bituminous. and the sulphurous.

He distinguished between the royal, base and semimetals; he discussed the different metals, their physical and chemical properties, their ores and extraction.

He dealt with the « analysis and assay of metallic ores ». In this analysis he displayed a systematic and extensive knowledge of qualitative analysis, he was acquainted with several of the

(1) KIRWAN always refers to nitric acid as nitrous acid.

tests, and methods of separation employed at the present time.

The book included tables of the composition of metallic calces, containing results of experiments carried out by BERGMAN, WENZEL, MORVEAU, TILLOT, GADOLIN, KLAPROTH, ELHUYART and himself. The results obtained by the different chemists are not in exact agreement, either with one another, or with KIRWAN's. He suggested various explanations of these inconsistencies.

« The present edition of these *Elements of Mineralogy* » says KIRWAN, « being grounded on a foundation so very different from the former, necessarily assumes a very different form, being much more copious, and as I hope, infinitely more perfect. The external characters (crystallisation excepted) are enumerated and described with very little variation from Mr. WERNER's tract on that subject; some new genera and many new species are introduced, the analysis by different persons annexed. Not slavishly addicted to any system I have taken the liberty when I thought it necessary, of framing new distinctions, and even of introducing a new class. I have also added the description of some Irish fossils, which appeared not to have been known in other countries. Volcanic productions that in the former edition were but slightly mentioned, will here be found minutely considered, and the origin of basalt fully discussed. I have also treated of the analysis of earths and stones much more at large than before, and impartially examined those already made; in a word I hope I have faithfully represented in this volume the present state of the science, as far as earths and stones are concerned. »

KIRWAN's *Analysis of Mineral Waters*, published in 1799, shows that the development of qualitative analysis was very pronounced during this period.

By mineral waters KIRWAN denoted such as either by their colour, taste, smell, or specific gravity, show an unusual impregnation with some mineral substances, or carbon dioxide. Apart from their importance in medicine he considered mineral waters as a valuable indication of the substances existing at unknown depths in the earth, and he pointed out that mineral waters have led to the discovery of several valuable ores.

« To state, add to, and generalize these improvements » (by FOURCROY, WESTRUMB, KLAPROTH, BLACK, GARNET, and PEARSON)

says KIRWAN, « by proposing new tests and new limitations of the powers of those already known, in cases where none were before determined, or where they were inaccurately assigned, also by substituting more direct methods of investigation to the random methods before employed, and various new modes of estimating the quantity of each of the substances discovered, is the purpose of the following sheets. »

KIRWAN gives the general tests for carbon dioxide, and the mineral acids. The reactions which he made use of as tests for baryta, lime, copper, sulphuretted hydrogen, etc., are those in favour at the present day.

The « *power* » of the different tests for sulphates and sulphuric acid was a matter of special investigation. He found a solution of muriated barytes to produce a very sensible precipitate in water containing $\frac{1}{9102.56}$ of its weight of real vitriolic acid ; « *acetited* » lead gave an immediate precipitate with water containing $\frac{1}{9099.0}$ of its weight of real vitriolic acid, and so on. Hence, in his opinion, by the successive application of these tests, we may determine nearly the proportion of vitriolic acid in waters that contain it « *singly* », and « *unsaturate* », and vice versa, by its means we may also form some conjecture of the proportion of the substance which it detects.

KIRWAN quoths experiments by CAVENDISH, BERGMAN, Dr. GARNET, TINGRY, WESTRUMB, ACCUM, CORNETTE, and MARET, to show « that many salts which are incompatible when in considerable proportions, may nevertheless co-exist when in a very minute proportion to the quantity of water that contains them. » He attributes the inactivity of the divellent powers in these cases « to the resistance of the particles of water to that motion and separation from each other, which the action of the divellent powers of the saline particles would necessarily induce. And hence when this resistance is diminished by heat a decomposition often takes place which otherwise would not. » Hence he thinks that the greater the attractive powers of the antagonist particles to each other, the greater must be their rarity before its activity can be counterbalanced by the resistance of the particles of water.

« The analysis of mineral waters, » says KIRWAN, « comprehends three branches of inquiry, viz., (1) the determination of the different species of salts contained in the mineral waters; (2) the estimation

of the quantity of each in a given quantity of water, either by weight, as in pounds, or by measures, as in a gallon, quart etc.; and lastly the determination of the species and quantity of elastic fluids contained in a free and uncombined state. »

No general reform of chemical language was proposed till the year 1781 when de MORVEAU in a memoir upon chemical denominations, set forth the necessity of improving the system, and the rules for attaining a perfect language. This memoir was accompanied by a Table of Chemical Nomenclature; containing the principal analogical denominations and examples of the formation of compound names. De MORVEAU was assisted by LAVOISIER, BERTHOLLET, and DE FOURCROY; and these four chemists, by their joint labours, compiled Tables of Chemical Nomenclature, according to the nomenclatural principles of De MORVEAU.

Dr. DICKSON and Mr. KIRWAN rejected the new system. The particular denominations themselves of the compound substances, seemed to be more objectionable in their opinion than the principle of naming them. Mr. KIRWAN thought that « new compounds should be expressed by compound denominations, expressing both members of the compound. But the denominations should be as few, as short, and as simple, as the exigency of the cases that demand their admission, will admit. » He was of the opinion that those names which expressed their respective objects by a single name appeared to have a considerable advantage. He argued that « the principle of denomination according to the composition applied unlimitedly, would lead us into needless and irksome prolixity. » Hence where compound substances have already obtained simple names, those names should be retained, as nitre, common salt, Borax, soap.

KIRWAN introduced some new chemical terms of his own;— muricaloite for limestones containing magnesia, and baryto calcite for limestones containing barytes; the stone called apatite by the Germans KIRWAN named phosphorite. The difficulty in forming a suitable name in one word for the substance called vegetable alkali induced KIRWAN to name it Tartarin. To him also we owe the conveniently abbreviated term oxy for oxygenated.

Technical chemistry made good progress during this period. The efforts of many eminent chemists, among whom we may mention KUNKEL, MARGRAF, and MACQUER were frequently directed to applying their scientific experiences of chemical processes to the advancement of particular branches of industry. KIRWAN also interested himself in chemistry applied to objects of immediate practical utility.

In volume 3 of the *Trans. of the Royal Irish Academy* he published a paper on « experiments on the Alkaline Substances used in Bleaching, and on the Colouring Matter of Linen-Yarn. » In this paper he gave an analysis of Barilla, which for the time was admirable. He described a method of estimating the alkali in alkaline substances. He pointed out the resinous nature of the colouring matter of linenyarn, determined its solubility in water, rectified spirit of wine and oil of turpentine; and established as he conceived the fact important from a national point of view :— that the linen manufacture of Ireland was altogether independent of foreign salts or ashes for the purpose of bleaching. KIRWAN's paper entitled « What are the Manures most advantageously applicable to the various sorts of Soils, and what are the causes of the beneficial effect in each particular instance » was a prize essay on the question proposed by the Royal Irish Academy. In it he distinguished between the different soils, and the manures best suited to each. He gave a table of contents of manures, the constitution of fertile soils, and the method of estimating their fertility. This paper is of great interest in the history of agricultural chemistry. It was published in 1802; before Sir HUMPHRY DAVY had begun to lecture on agricultural chemistry, two years before DE SAUSSURE's work was published, and more than thirty years before LIEBIG's time.

In 1784 CAVENDISH wrote a paper entitled « Experiments on Air. » Later in the same year KIRWAN wrote—« Remarks on Mr. CAVENDISH's Experiments on Air ». This was followed by « Mr. CAVENDISH's Answer to the foregoing remarks », and later by « Mr. KIRWAN's reply to Mr. CAVENDISH's answer ». KIRWAN attributed the diminution of respirable air, observed in common phlogistic processes, « to the generation and absorption

of fixed air, which is now known to be an acid, and capable of being absorbed by several substances ».

CAVENDISH expressed the opinion that the diminution of respirable air was owing to the formation of water, and that fixed air was never produced in phlogistic processes, except when it may be attributed to the decomposition of some animal or vegetable substance present.

KIRWAN held that on the calcination of metals fixed air was formed by the union of the dephlogisticated part of common air with the phlogiston of the metal. CAVENDISH allowed the fact in general but ascribed the fixed air to the exposure to the atmosphere. KIRWAN denied that the quantity of fixed air in the atmosphere is appreciable.

In 1786 KIRWAN published a paper on « Experiments on Hepatic Air » in the *Philosophical Transactions*. Here he described methods of preparing Sulphuretted Hydrogen; he pointed out its acid properties, its combination with alkalies, its effect on metals. He determined the « absolute weight » of the gas by weighing it in a glass bottle previously exhausted. He observed that a mixture of one part hepatic air, and 1.5 of common air burned blue without flashing or detonating. He remarked that during the combustion sulphur is constantly deposited, and a smell of vitriolic air is perceived. He carried out experiments on mixtures of hepatic air and other gases. In this paper also he gave an account of Phosphoretted Hydrogen gas, of which he was one of the original discoverers.

In the *Trans. of the R. I. Academy* dated Jan. 9 1794 we find « Experiments on a new earth found near Stronthian, in Scotland », by R. KIRWAN, in which, having referred to Dr. CRAWFORD's experiments on it in the year 1790, and also the account of its chemical properties given by Sulzer, he said that it appeared to him from his experiments to be a new earth intermediate between barytes and lime. Mr. KIRWAN may be looked upon as one of the discovers of this mineral.

In volume 6 of the *Trans. of the R. I. Academy*, KIRWAN wrote a paper entitled « Of the Composition and Proportion of Carbon in Bitumens and Mineral Coal ». The method of estimation he employed was that of fusing with nitre. The various specimens

of coal he examined were: Kilkenny coal, Maltha, Asphalt, Cannel coal, Whitehaven coal, Wigan coal, Swansea coal and Leitrim coal.

KIRWAN's paper « Thoughts on Magnetism » published in 1796, contained some excellent observations on crystallisation; which he considered an analogous power to magnetism.

As evidence of Mr. KIRWAN's genius and industry we need only refer to his numerous publications, and the unusual diversity of their subject.

His works may be comprised under the heads of Divinity, Metaphysics, Logic, Law, Philology, Music, Mechanical Philosophy, Chemistry, Mineralogy, Mining, Geology, Meteorology.

Of his papers, six were published in the *Philosophical Transactions* 1781-1786, the titles are as follows: —

1. Experiments and observations on the Specific Gravity and Attractive Powers of various saline substances. 1781
2. Continuation of the same subject. 1782
3. Conclusion of the same subject. 1783
4. Remarks on Mr. CAVENDISH's Experiments on Air. 1784
5. His reply to Mr. CAVENDISH's answer. 1784
6. Experiments on Hepatic Air. 1786

To the *Transactions of the Royal Irish Academy*, between 1788 and 1809, he contributed 38 memoirs as follows: —

1. Essay on the Variations of the Barometer. 1788
2. Observations on Coal Mines. 1789
3. Experiments on the Alkaline Substances used in bleaching and on the colouring matter of Linen-Yarn. 1789
4. On the strength of acids and the proportion of ingredients in neutral salts. 1790
5. A comparative view of meteorological observations made in Ireland since the year 1788 with some hints towards forming prognostics of the weather. 1793
6. Reflections on meteorological Tables, ascertaining the precise signification of the terms Wet, Dry, and Variable. 1793
7. State of the weather in Dublin from June I. 1791 to June I. 1793. (date unknown)

8. Examination of the supposed igneous origin of stony substances. 1793
9. What are the manures most advantageously applicable to the various sorts of soils, and what are the causes of the beneficial effect in each particular instance. 1794
10. Meteorological observations made in Ireland. 1794
11. Experiments on a new earth found near Stronthian. 1794
12. On the composition and proportion of carbon in bitumens and mineral coal. 1795
13. Synoptical view of the state of the weather in Dublin. 1796
14. Thoughts on magnetism. 1796
15. On the primitive state of the globe and its subsequent catastrophe. 1796
16. Synoptical view of the state of the weather. 1796
17. Synoptical view etc. 1796
18. Additional observations on the proportion of real acid in the three ancient known mineral acids, and on the ingredients in various neutral salts and other-compounds. 1797
19. Essay on human liberty. 1798
20. Synoptical view etc. 1798
21. Synoptical view etc. 1799
22. Observations on the proofs of the Huttonian Theory of the earth. 1800
23. An illustration and confirmation of some facts mentioned in an essay on the primitive state of the globe. 1800
24. An essay on the declivity of mountains. 1800
25. On chemical and mineralogical nomenclature. 1800
26. Remarks on some sceptical positions in Mr. HUME's inquiry concerning the human understanding, and his treatise on human nature. 1800
27. Synoptical view of the weather in Dublin. 1800
28. On the variations of the atmosphere. 1801
29. Synoptical view etc. 1801
30. Synoptical view etc. 1802
31. Synoptical view etc. 1803
32. On space and duration; of duration; time, and eternity. 1805
33. Synoptical table. 1805
34. On the primaeval language of mankind. 1805
35. Description of a new Aneometer. 1808
36. Synoptical view, continued to 1808. 1808
37. An essay on happiness. 1809
38. On the origin of Polytheism, Idolatory, and Grecian Mythology. 1809

Some of these essays were later much enlarged and published in independent volumes. His other works include, besides those previously mentioned, a Criticism of the Huttonian Theory of the earth, An Estimate of the temperature of different latitudes (1787), Geological Essays (1799), Logick; or an essay on the elements... of reasoning (1807), An Essay on Human Happiness (1810). He published also a volume of Metaphysical Essays, and a few copies of a treatise on the interpretation of the Apocalypse. He wrote but did not publish a treatise on Music. He had also a treatise partly written, entitled « Commentaries on LOCKE's Essay on The Human Understanding ».

That KIRWAN was appreciated throughout the civilized world, is evident from the honours conferred upon him in foreign countries and in his own. He was honorary member of the Academies of Stockholm, Upsala, Berlin, Dijon, Phidelphia, and of the Mineralogical Society of Jena; he was Fellow of the Royal Societies of London and Edinburgh, and honorary member of the Manchester Society. He was President of Royal Irish Academy, 1799-1812. In his latter days a Chemical and Mineralogical society formed in Dublin was after him called the Kirwanian Society, and of this he was President. He was also President of the Dublin Library Society. He had the honorary title of Inspector-General of his Majesty's mines in Ireland. He was elected Perpetual Member of the Amicable Society of Galway. From the University of Dublin he received the degree of Doctor of Laws, and was always allowed a seat at the examination for Fellowship. By some of the French savants he was designated « the Philosopher of Dublin ». He was consulted by half the farmers of Ireland as a weather-prophet. The Empress CATHERINE THE SECOND of Russia presented him with her portrait, as a token of her esteem. In 1802 he was asked by the Society of the R. I. Academy to sit to HUGH D. HAMILTON for his portrait, which was to be hung in the Museum in honour of « his eminent services and indefatigable labours in Chemistry and Mineralogy etc. »

In his translation of Chemical Nomenclature Dr. PEARSON dedicated that work to Mr. KIRWAN in the following letter :—

To RICHARD KIRWAN ESQ.

Dear Sir,

The dedication of an elementary work calculated for students,

cannot procure you any additional respect from the learned world; for by your ceaseless researches, the boundaries of natural knowledge have been so enlarged, that the present and future ages must hail you as a Benefactor of the human race. Still some grateful acknowledgments are due from those who, enlightened by your science, and actuated by your virtuous example, pursue the same course, although not with equal speed. The present offering I pray you, Sir, to accept; confiding that he who displayed such magnanimity in the renunciation of a doctrine which had been ably defended by him for ten whole years, against a host of consummate masters in the science, cannot be displeased with me for differing in opinion on a few points, of not however, any great importance. With the highest reverence for your distinguished virtues, I ever shall remain,

Dear Sir,

Your devoted servant,
And affectionate friend,
GEORGE PEARSON.

This great tribute to KIRWAN's genius, from a contemporary scientist shows the high esteem in which he was held even by the people of his own day.

(University College, Cork, Ireland)

J. REILLY and N. O' FLYNN.

John William Schibbye Johnsson

The Danish historian of medicine, J. W. S. JOHNSSON (Aug. 22, 1868-Feb. 4, 1929), was personally well known and highly appreciated for his active participation in numerous congresses and for his efforts to further international collaboration in the world of science. His mastery of several languages was of great help to him in this work.

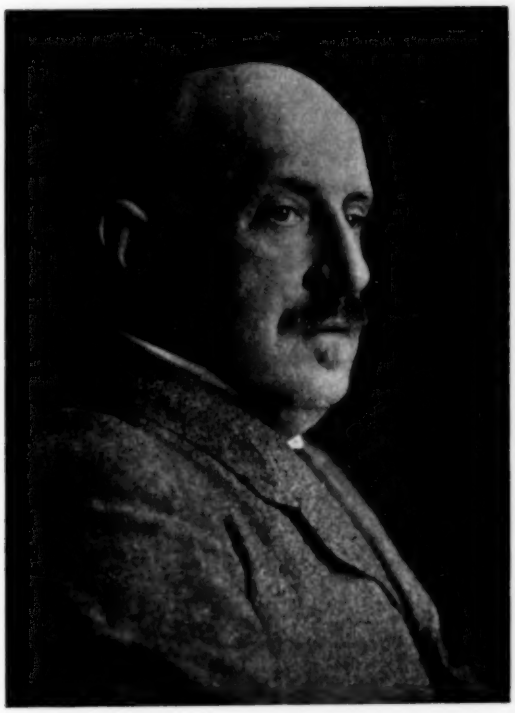
His industrious life was equally divided between his practice in Copenhagen (Westend) and medico-historical research. He was the beloved family-physician, highly esteemed by his patients as well as by fellow-physicians; and he published a number of studies of lasting value—mostly papers on simple subjects, but also some larger works.

A beautiful outcome of his love for medico-historical research was his never-tiring readiness to help, by word and deed, anyone who came to him for advice; and he made many friends who were indebted to him in that way.

Early in life his interest in medical folk-lore and biography lead him on to medico-historical research. He gradually extended the scope of his studies, and he became an authority in the interpretation of manuscripts from olden times, particularly the middle ages. He edited several of these manuscripts—for instance, HENRICUS DE DACIA: *De simplicibus medicinis laxativis*. In this work he pointed out the circumstances which indicate that the author is identical with the most famous Danish physician in the middle ages, HENRIK HARPESTRENG, who wrote his principal works in Danish. By this study JOHNSSON increased our knowledge of the northernmost offshoots of the Salernitan school.

He also did some very significant work on the history of the plague in Italy and Dalmatia, publishing documents which give important and authentic information on this subject. Some of these papers are well known to the readers of « *Isis* ».

Many of his papers, and particularly his larger works, are of



JOHN WILLIAM SCHIBBYE JOHNSSON
(1868-1929)

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UNIV. OF MICH.

special importance for the medical history of Denmark and Norway —as, for instance, his elaborate study on Danish medical societies which helps to explain the progress of medical science in the past 150 years.

As is evident from the list of his publications, he preferred the study of single subjects to the composition of wide ranging surveys. This preference was connected with a marked tendency to self-criticism which kept him from planning more than he could master. His research covered some wide fields in the history of medicine—he did not take up with classical antiquity, though—but his most valuable contributions are his studies on medical folk-lore. Unfortunately, his sudden death broke off several investigations he had already started, in particular one on Danish folk-medicine.

He was a joint founder of the Medico-historical Museum in Copenhagen and of the Danish Society of Medical History. Indeed he was a born organizer. Inspired by his love for humanity and stimulated by his wife, who was of Austrian birth, he took considerable pains to organize Danish aid for Austrian children after the world war.

He met his death in his working field, being infected by a patient with influenza. A fine death for a man of his type. But, several problems he had taken up will remain unsettled for some time.

His loss will be deeply felt not only by his friends but also by anyone interested in Danish medical history and in the history of science.

(*Kolding, Denmark*) AXEL HANSEN.

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Greek Theorists of Music in Arabic Translation

It is worthy of notice that whilst Western Europe of the Middle Ages was almost entirely ignorant of the works of the Greek musical theorists (save what could be gleaned from MARTIANUS CAPELLA, BOËTHIUS, CASSIODORUS, & ISIDORE OF SEVILLE), the Arabs were conversant with the treatise of ARISTOXENUS, ARISTOTLE, EUCLID, NICOMACHUS, and PTOLEMY. The information that has been preserved in Arabic works concerning the above Greek writers enables us to throw fresh light on some of their works. Further, it is highly probable that when the works of the Arab theorists (1), who borrowed considerably from the Greeks, have been edited with a substantial *apparatus criticus*, many a debateable word or passage in the Greek treatises may be cleared up.

To deal with this question within the limits of a short paper it is perhaps best that it should be approached under two headings, —(a) the Greek theorists, and (b) the Arab theorists.

THE GREEK THEORISTS.

PYTHAGORAS is mentioned by IBN AL-QIFṬÎ (d. 1248) as the author of a treatise on music which had been translated into Arabic (2). Following the Greeks, PYTHAGORAS was venerated by the Arab theorists as the founder of the theory of music (3), although we get little actual « theory » quoted under his name.

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(1) I use the term « Arab » advisedly, just as I would use the word « English », at the same time implying the Scots, Irish, & Welsh. « Islāmic » or « Muslim » will not serve, because Magians, Jews, & Christians, contributed to this « Arabian culture ». « Saracenic » is equally undesirable, because it includes the 'Uthmānlī Turks, & further, it is meaningless in the Orient.

(2) IBN AL-QIFṬÎ, 259.

(3) IKHWĀN AL-ṢAFĀ' (Bombay Edit.), I, 102. ḤAJJĪ KHALĪFA, VI, 255. FAKHR AL-DIN AL-RĀZĪ, *Brit. Mus. MS.*, Or. 2972, fol. 154v.

He is mostly drawn upon, as is HERMES, for opinions on the moral influence of music.

PLATO, according to IBN AL-QIFṬÎ, wrote on music in his youth, a statement that does not find a place in any Greek documents extant. All that we know of PLATO's writings on music is contained in the *Timaeus* (35-6, 47, 67), which was translated into Arabic by YÛḤANNÂ IBN AL-BAṬRÎQ (d. 815), and again by ḤUNAIN IBN ISḤÂQ (d. 873).

ARISTOTLE had a tremendous influence on the Arabian culture movement. Among the Aristotelian writings dealing with the speculative theory of music, the Arabs possessed the *Kitâb al-masâ'il (Problemata)*, the *Kitâb fî'l-nafs (De anima)*, and the *Kitâb al-ḥayawân (Historia animalium)*. The *Problemata*, whose XIXth section deals with music, was translated into Arabic by ḤUNAIN IBN ISḤÂQ, who was also responsible for *De anima*, which contains a chapter on « sound ». As for the *Historia animalium*, it was translated by YÛḤANNÂ IBN AL-BAṬRÎQ, and again by ABÛ BISHR MATTÂ (d. 940). The work attributed to ARISTOTLE by WENRICH, entitled *Tâsîs mûsîqâ* (μουσική) (4), would appear to be a copyist's error for *Thâsîs fûsîqâ* (φυσική) (5). Another work appearing under ARISTOTLE's name in Arabic is a *Kitâb quwwal al-ḥukamâ' fî'l-mûsîqî (Dicta of the Philosophers on Music)*.

Among the commentaries on *De anima* by Greek writers that appeared in Arabic were those of THEMISTIUS, SIMPLICIUS, and ALEXANDER APHRODISIENSIS. The first and last were translated into Arabic by ḤUNAIN IBN ISḤÂQ. A work by GALEN, entitled in Arabic the *Kitâb al-ṣaut (De voce)* was also translated by ḤUNAIN. It was such treatises as these that schooled the Arab authors in the physical & physiological aspects of the theory of sound.

Several works have come down to us in Arabic on organs and similar devices bearing the names of ARCHIMEDES, APOLLONIUS PERGAEUS, and a certain MÛRṬUS or MÛRISṬUS, of which we have no Greek texts extant. More important perhaps, were the borrowings of the Arabs from the next group of writers.

ARISTOXENUS, according to the *Fihrist* (*ca.* 988), was known

(4) WENRICH. *De auct. Graec.*, 152.
(5) IBN AL-QIFṬÎ, 46.

in Arabic by two works,—a *Kitâb al-rîmûs* and a *Kitâb al-îqâ'* (6) The former is evidently a copyist's error for *Kitâb al-ru'ûs*, which would confirm the suggestions of MARQUARD (7), WESTPHAL (8), and GEVAERT (9), that the *'Αρμονικὰ στοιχεῖα* of ARISTOXENUS that has come down to us is actually made up of two distinct works,—the *ἀρχαί* (= *ru'ûs*), and the *στοιχεῖα*. As for the *Kitâb al-îqâ'* *(Book of Rhythm)*, it is clearly the work on rhythm that we now possess in the fragmentary pieces collected from the Greek by WESTPHAL (10).

EUCLID appears to have had two works on music attached to his name in Arabic. In the *Fihrist* and IBN AL-QIFṬÎ these are called a *Kitâb al-nagham* and a *Kitâb al-qânûn* (11). The former is probably the Pseudo-Euclidian tract known as the *Εἰσαγωγὴ ἁρμονική* attributed to CLEONIDES. The second treatise is evidently the *Κατατομὴ κανόνος*. Here an interesting point arises. It appears that in no ancient Greek codex are these works ascribed to EUCLID (12), and, apparently, no writer earlier than PORPHYRY credits EUCLID with the authorship of the former (13). The *Fihrist* shows however, that the sources of the Arabic versions must have carried EUCLID's name, or else that the translator or the author of the *Fihrist*, must have known of the PORPHYRY reference in one case.

NICOMACHUS, says the author of the *Fihrist*, was known in Arabic by a *Kitâb al-mûsîqî al-kabîr (Opus major on music)*, and by several compendia *(mukhtaṣarât)* of this work (14). This information would seem to give us partial confirmation of the opinion put forward by VON JAN and others (15), that NICOMACHUS actually compiled the larger and more detailed work as promised in his *'Εγχειρίδιον ἁρμονικῆς*, since the Arabic title, *Kitâb al-mûsîqî al-kabîr*, might conceivably conform with the « larger

(6) *Fihrist*, I, 270.

(7) *Die harmonische Fragmente des Aristoxenus* (1868).

(8) *Metrik*, I, 36-43.

(9) *La musique dans l'Antiquité*, I, 8. See also MACRAN, *The Harmonics of Aristoxenus*, 86 et seq., and RUELLE, *Les Éléments harmoniques d'Aristoxène*.

(10) *Die Fragmente u. Lehrsätze der griechischen Rhythmiker* (1861).

(11) *Fihrist*, I, 266. Ibn al-Qifṭî, 65.

(12) *Ency. Brit.*, (xith edit.), IX, 879.

(13) WALLIS, *Op. Math.*, (1699), III, 267.

(14) *Fihrist*, I, 269.

(15) VON JAN, *Musici scriptores Graeci*.

work » that NICOMACHUS himself refers to. Probably, the ἐγχειρίδιον (= *mukhtaṣar*) that has come down to us is merely one of the compendia mentioned above. The *Arithmetic* of NICOMACHUS, which also deals with musical intervals, the canon, and the harmonic mean, was also known in Arabic in the translation of THÂBIT IBN QURRA (d. 901).

PTOLEMY, although ignored as a musical theorist in the *Fihrist*, and by IBN AL-QIFṬÎ and IBN ABÎ UṢAIBI'A, is mentioned by IBN 'ABD RABBIHI (d. 940) (16), AL-MAS'ÛDÎ (d. *ca.* 956) (17), and the IKHWÂN AL-ṢAFÂ' (10th cent.) (18) as such, and the *Kitâb al-mûsîqî* (Book of Music) referred to is probably the work on τὰ ἁρμονικά that we now possess in Greek. From AL-MAQQARÎ (d. 1632) it might be assumed that the famous Andalusian musician ZIRYÂB (9th cent.) was familiar with the *Kitâb al-mûsîqî* of PTOLEMY (19).

When and by whom these works of ARISTOXENUS, EUCLID, NICOMACHUS, and PTOLEMY were translated into Arabic, we have no definite information, and not a solitary exemplar of these works have been preserved in Arabic. A « clue » to the date occurs in the *Kitâb al-aghânî*, where the celebrated ISḤÂQ AL-MAUṢILÎ (d. 850) speaks of books on music by the « Ancients » *(awâ'il)*, i.e. the Greeks, that were being translated into Arabic for MUḤAMMAD IBN AL-ḤASAN [IBN MUṢ'AB], and we know from internal evidence that the incident related probably took place before the year 847.

The four great translators who were interested in music were ḤUNAIN IBN ISḤÂQ (d. 873), AL-KINDÎ (d. *ca.* 874), THÂBIT IBN QURRA (d. 901), and QUSṬÂ IBN LÛQÂ (d. 932). It was these savants who were probably responsible for some of the above translations. ḤUNAIN IBN ISḤÂQ wrote a work on *The Dicta of the Philosophers* which contained a section entitled,—*Ijtimâ'ât al-falâsifa wa nawâdirhum fî'l-alḥân wa'l-mûsîqî (Dicta of the Philosophers concerning Melodies and Music)*. The Arabic version has been preserved (20), and in it PYTHAGORAS, PLATO, ARISTOTLE,

(16) *'Iqd al- farîd* (Cairo edit.), III, 186.
(17) *Kitâb al-tanbîh* (Bibl. Geog. Arab.), VII, 128.
(18) I, 102.
(19) *Analectes*, II, 87.
(20) *Munich MS.*, Arab. 651.

EUCLID, HERMES, and others are quoted, including a certain FANLUDÛS, who would appear to be identical with the individual named FANDURÛS or QANDHURÛS by IBN KHURDÂDHBIH (d. 912) (21). This work was also translated from the Arabic into Hebrew by AL-ḤARIZÎ (13th cent.) (22).

THE ARAB THEORISTS.

Although no translations in Arabic of the Greek musical treatises of ARISTOXENUS, EUCLID, NICOMACHUS, or PTOLEMY have seemingly been preserved, yet the works of AL-KINDÎ, AL-FÂRÂBÎ, the IKHWÂN AL-ṢAFÂ', ABÛ 'ABDALLAH MUḤAMMAD AL-KHWÂRIZMÎ, IBN SÎNÂ, IBN ZAILA, ṢAFÎ AL-DÎN 'ABD AL-MU'MIN, AL-SHÎRÂZÎ and others, demonstrate how assiduously these translations, and even perhaps the Greek originals themselves, must have been studied.

AL-KINDÎ (d. *ca.* 874) openly acknowledged that he followed the « Ancients » i.e. the Greeks, in the speculative theory of music. The four (?) extant treatises on music that are attributed to him (23), were probably written during the reign of AL-MU'TAṢIM (833-42), and certainly before the close of the reign of AL-MUTAWAKKIL (847-61). The question has been argued more than once whether AL-KINDÎ derived his knowledge of the Greek sciences from Greek texts or from translations (24). In one of the above treatises AL-KINDÎ refers to the *qîthûra (cithara)*, which would appear to show that he was depending in this instance on a Syriac authority (25). Although AL-KINDÎ deals with the theory of sound *(ṣaut)*, intervals *(ab'ad)*, genres *(ajnâs)*, systems *(jumû')* and species *(anwâ')*, modes *(luḥûn)*, tones *(ṭanînât)*, mutation *(intiqâl)*, and composition *(ta'lîf)*, as do the Greeks, yet it is difficult to locate his precise sources. He was evidently

(21) See my article « IBN KHURDÂDHBIH on Musical Instruments » in the *JRAS.*, July, 1928.

(22) The Hebrew text *(Musare ha-filosofim)* was annexed to the work of ABEN GABIROL entitled *Takkun middot ha-nefesh* (Riva di Trento, 1562). See HUNAIN IBN ISCHĀQS *Sinnsprüche der Philosophen, nach der hebr. Uebers. von Charisi ins Deutsche uebertr. von* A. LOEWENTHAL. (Berlin, 1896).

(23) See my article in the *JRAS.*, Jan. 1926, « Some Musical MSS. Identified ».

(24) LECLERC, *Hist. de la médecine arabe*, I, 135.

(25) *Berlin MS.* (Ahlwardt) 5531, fol. 24. He uses the word *qîthâra* however in the *Brit. Mus. MS.*, Or. 2361.

acquainted with EUCLID and apparently with PTOLEMY. At the same time, many of his opinions on the physical and physiological aspects of the theory of sound, may be quite original (26). He was endowed with exceptional abilities, and was sufficiently equipped on the mathematical side to contribute commentaries on and rectifications of EUCLID's *Elements*.

AL-FÂRÂBÎ (d. *ca.* 950) depended on sources that were clearly different from those consulted by AL-KINDÎ. Indeed, AL-FÂRÂBÎ wrote his famous *Kitâb al-mûsîqî al-kabîr* because he was dissatisfied with what had been handed down from the Greek theorists in Arabic translation. He found *lacunae* as well as obscurities in the latter, and as he thought too much of the « Ancients » to blame them for these shortcomings, he attributed the blemishes to the copyists (27). AL-FÂRÂBÎ's chief authorities were EUCLID, PTOLEMY, and THEMISTIUS. The « Commentary » *(sharḥ)* on *De anima* by the latter, dealt perspicuously with the theory of sound, and AL-FÂRÂBÎ himself wrote a « Commentary » on this « Commentary » (28). A modern author has expressed the opinion that AL-FÂRÂBÎ was probably acquainted with ARISTIDES QUINTILIANUS, but this is improbable. (29) The order in which the various divisions of Greek theory are discussed by AL-FÂRÂBÎ, shows that EUCLID was the principal authority (30).

The IKHWÂN AL-ṢAFÂ' (10th cent.) were thorough-going Aristotelians, and were acquainted with the writings of NICOMACHUS, PTOLEMY, EUCLID, and others. They appear to have borrowed from AL-KINDÎ (or from his sources) since they make the same errors as the latter. They also use the *Dicta of the Philosophers* of ḤUNAIN IBN ISḤÂQ. According to the « Brethren », PYTHAGORAS was the founder of the theory of music, and they say that *after him* came NICOMACHUS, PTOLEMY, and EUCLID, from which it might be assumed that the IKHWÂN recognized the late composition of the treatises ascribed to EUCLID. The theory of the IKHWÂN concerning the spherical propagation of sound is interesting.

(26) *Berlin MS.* (Ahlwardt), 5531, fol. 23. See my *History of Arabian Music*, pp. 127-8.

(27) *Leyden MS.*, Or. 651, fol. 2.

(28) WENRICH, *op. cit.*, 279.

(29) *Zeit. Inter. Mus. Gesell.*

(30) ARISTIDES, like ALYPIUS and MARTIANUS CAPELLA, takes the *genres* after the *systems*.

It does not occur in any of the above-mentioned Greek writers, although it may be found in VITRUVIUS (31). Did the IKHWÂN borrow it from an unknown Greek source, or was it original with them? (32).

ABÛ 'ABDALLAH MUḤAMMAD AL-KHWÂRIZMÎ (10th cent.), the author of the *Mafâtîḥ al-'ulûm (Keys of the Sciences)*, deals with Greek theory. His nomenclature reveals that he consulted sources other than AL-KINDÎ and AL-FÂRÂBÎ. His reference to Byzantine musical instruments is interesting since direct information concerning Byzantine music at this period is lacking (33).

IBN SÎNÂ (d. 1037) is claimed by IBN AL-QIFṬÎ to have dealt with aspects of the theory of music that had been neglected by the « Ancients ». Besides a lengthy treatment of the subject in his monumental work *Al-shifâ'* and his smaller treatise *Al-najât*, IBN SÎNÂ wrote separate works on musical theory. It has been assumed by CASIRI (34) and WENRICH (35) that he wrote an abridgement of EUCLID on music. Both these writers appear to have misread IBN AL-QIFṬÎ on this point. The latter merely says that IBN SÎNÂ abridged the science of EUCLID (i.e. geometry), and arithmetic, and music (36). IBN SÎNÂ worked independently of the writings of AL-FÂRÂBÎ, and his reading of Greek theory is of considerable interest.

IBN AL-HAITHAM (d. 1038), according to IBN ABÎ UṢAIBI'A, was the author of a *Sharḥ al-qânûn (Commentary of the Canon)* and a *Sharḥ al-rumûnîqî* (37). These two works must refer to the Pseudo-Euclidian treatises, since the former is entitled by IBN AL-QIFṬÎ a *Sharḥ qânûn Uqlaidis (Commentary on the Κατατομὴ κανόνος of Euclid)*, whilst the latter is evidently a copyist's slip for *Sharḥ al-armûnîqî (Commentary on the Εἰσαγωγὴ ἁρμονική)*.

IBN ZAILA (d. 1048) was a pupil of IBN SÎNÂ. His *Kitâb al-kâfî fî'l-mûsîqî*, a solitary copy of which is preserved in the British

(31) *De architectura*, V, 3. See my *Facts for the Arabian Musical Influence*, Append. 33, and my *Arabic Musical MSS. in the Bodleian Library*, pp. 4-6.
(32) IKHWÂN AL-ṢAFÂ' (Bombay edit.), I, 88.
(33) *Liber Mafâtîḥ al-Olûm* (Leyden, 1895), 236-8.
(34) *Bibl. arab.-hisp. Escur.*, I, 271.
(35) *op. cit.*, 189.
(36) IBN AL-QIFṬÎ, 421.
(37) IBN AL-QIFṬÎ, 168. IBN ABÎ USAIBI'A, II, 90.

Museum, shows that he followed his master rather slavishly in most things regarding Greek theory, although he is somewhat independent when dealing with Arab theory. He uses a notation which is to be found in the *Ἐγχειρίδιον ἁρμονικῆς* of NICOMACHUS (38).

ṢAFÎ AL-DÎN 'ABD AL-MU'MIN (d. 1294) was acquainted with, and quotes from, both AL-FÂRÂBÎ and IBN SÎNÂ. As librarian to AL-MUSTA'ṢIM, the last khalif of Baghdâd, he probably had the opportunity of consulting the original translations from the Greek that had been made in the 8th, 9th, 10th centuries, especially in the *Bait al-ḥikma* or « College of Science », which appear to have been still preserved in the khalif's library (39). It is from ṢAFÎ AL-DÎN that we learn that the Arabs used the Nicomachian notation for recording their melodies (40).

Although the Mughâl incursions, which brought about the destruction of the great libraries in the Islâmic East, and the vandalism of both the Muslims and Christians in Spain, probably robbed us of most of the works of the Greeks on music in Arabic, yet some of them appear to have survived. This is evident from AL-SHÎRÂZÎ (d. 1310) (41), the *Muḥammad ibn Murâd MS.* (42), the *Sharḥ maulânâ* (15th cent.) (43), and SHAMS AL-DÎN AL-ṢAIDÂWÎ (16th cent. MS.) (44). In the *Sharḥ maulânâ* there is an archaic Greek notation, somewhat similar to the *σημεῖα* in ALYPIUS. SHAMS AL-DÎN AL-ṢAIDÂWÎ records melodies by means of a stave. The system is not unlike the Greek (? Byzantine) specimens given by GALILEI (45), and KIRCHER (46). In view of this, the suggestion of fraud which has been laid at the door of KIRCHER ought to be withdrawn (47). At any rate,

(38) *Brit. Mus. MS.*, Or. 2361, fol. 226v, 236. MEIBOM, *Ant. Mus. Auct.*, I, III, 39-40.
(39) *Ency. of Islâm*, III.
(40) *Brit. Mus. MS.*, Or. 136, fol. 38v.
(41) *Brit. Mus. MS.* Add. 7694.
(42) *Brit. Mus. MS.*, Or. 2361, fol. 168v.
(43) *Brit. Mus. MS.*, Or. 2361, fol. 131v.
(44) *Bodleian MS.*, Marsh, 82.
(45) *Dialogo della Musica antica* (1581), p. 36.
(46) *Musurgia universalis* (1650), I, 213.
(47) *Revue des Études grecques*, V, 266.

the existence of the Arabic counterpart opens up an interesting point. Did the Arabs borrow this particular system from the Byzantines, or ought we to assume that the adoption proceeded *vice versâ*?

(Glasgow) HENRY GEORGE FARMER.

Ueber die Entstehung der Messkunst.

Das Studium der Messkunst bei Naturvölkern und primitiven Völkern führt zu einigen, wie mir scheint, für die Geschichte der Mathematik nicht wertlosen Ergebnissen.

Zunächst wird, allerdings mit Einschränkungen, die Vermutung TYLORS bestätigt, dass die Messkunst sich, ebenso wie die Rechenkunst, am menschlichen Körper entwickelte. (1) Weiter erkennt man, in wie ausserordentlich engem Anschluss an die Einzelheiten der Messgelegenheit oder des Messbedürfnisses ursprünglich die Messkunst entstand. Dann kann man ferner feststellen, dass ebenso wie auf die Entwicklung der Rechenkunst auch auf die Entwicklung der Messkunst ganz ausserhalb der Sache liegende Faktoren Einfluss gewinnen konnten, und schliesslich erkennt man den Einfluss, den auf primitiven Stufen die mangelhafte Entwicklung der Rechenkunst auf die Entwicklung der Messkunst gewinnen konnte.

Ich will all dies an einigen wenigen Beispielen erörtern :

Bei den Wachaga am Kilimandscharo sind auf dem Markt die Längenmasse die Hand und die Unterarmlänge (2). Auch die Basoga-Batamba in Uganda stellen die Länge von allem, was sie messen müssen, mit dem « mukono », wörtlich « Arm » fest.

Damit ist gemeint die Länge vom Ellenbogen bis zur Spitze des Mittelfingers. (3) Die Indianerstämme des Pugetsundes benutzten beim Handel auf Schnüren aufgezogenes Muschelgeld. Die Länge der Schnüre wurde gemessen und zwar mit der Entfernung von der Brustmitte bis zum Ende des Daumens am ausgestreckten Arm und mit der Entfernung zwischen den Daumen-

(1) TYLOR, Einleitung in das Studium der Anthropologie und Civilisation, Deutsche Ausgabe, Braunschweig 1883; S. 380.

(2) P. E. MEYER, Le Kirengo des Wachaga; *Anthropos* 1917-18, S. 194.

(3) F. CONDON, Contribution to the ethnography of the Basoga-Batamba, *Anthropos*, 1911, S. 367.

spitzen der beiden möglichst weit auseinander gestreckten Arme (4). Die Eingeborenen von Sarawak (5) auf Borneo benutzen als Längenmasse die Breite der Finger, was übrigens HUREL auch von den Bakerewe (6) in Afrika schreibt. Vier Finger bilden dann in Sarawak eine Handbreite, das nächstgrössere Mass ist die Spanne. Man unterscheidet zwei Spannen, nämlich die Entfernung zwischen der Spitze des Daumens und der Spitze des möglichst weit davon abgestreckten Zeigefingers und zwischen der Spitze des Daumens und der Spitze des Mittelfingers. Die Norpapua (7) (Murik-Kaup-Karau) bei Dallmannhafen auf Neuguinea stellen die Länge eines Baumstammes entweder mit Hilfe einer Schnur fest oder dadurch, dass ein Mann mit möglichst weit auseinander gestreckten Armen und Händen von der Handspitze der rechten bis zur Handspitze der linken Hand den Baum abtastet. Um die Dicke eines Baumes zu messen, z. B. um festzustellen, ob zwei Pfosten eines Hauses die gleiche Dicke haben, benutzen sie auch eine Schnur. Die Armspanne ist auch das gebräuchlichste Mass bei den Bewohnern des nördlichen Teiles der Gazellehalbinsel und der vorgelagerten Inseln in der ehemals deutschen Südseekolonie (8). Das kleinste Mass für Längen auf der Insel Yap (9) in der Südsee ist die Dicke des Fingers « Vugul ». Weiter unterscheidet man auf Yap genau wie in Sarawak die beiden genannten Handspannen. Die kürzere heisst « de fon e gidi », « den Menschen gehörig », die längere, also von der Spitze des Daumens bis zur Mittelfingerspitze « de fon e kan », das heisst, « den Dämonen gehörig ». Schiesslich unterscheidet man auf Yap zwei verschiedene Armspannen oder Klaftern. Bei der einen « toi in dschangak » sind die Arme möglichst weit auseinandergestreckt und die Hände geöffnet, bei der anderen « toi in mokol » sind die Hände zur Faust geschlossen. Auch grössere Entfernungen werden vielfach bei Naturvölkern und primitiven

(4) HAEBERLIN, Indianerstämme des Pugetsundes, *Zeitschrift für Ethnologie*, 1924.

(5) LING ROTH, The Natives of Sarawak, II, S. 240.

(6) P. HUREL, Religion et vie domestique des Bakerewe, *Anthropos*, 1911, S. 277.

(7) I. I. SCHMIDT, Die Ethnographie der Nor-Papua, *Anthropos*, 1923-24 S. 724.

(8) P. O. MEIER, Fischerei bei den Uferleuten des nördlichen Teils der Gazellehalbinsel und speziell auf der Insel Vuatam, *Anthropos*, 1913, S. 328.

(9) MÜLLER, Yap, Hamburg 1917, S. 126 (Ergebnisse der Südsee-Expedition 1908-1910).

Völkern noch insofern durch Beziehung auf den menschlichen Körper gemessen, als sie in Tagereisen angegeben werden und dabei eben die Vorstellung eines gesunden kräftigen Mannes zugrunde liegt. Als ERLAND NORDENSKIÖLD 1913 im Dorf des Huanyamindianerhäuptlings Pedro fragte, wie weit es sei bis zur nächsten Niederlassung der Huanyamindianer, da antwortete man ihm: « Trumi, trumi, trumi ..... » zehnmal mit hintereinander aufgehobenen Fingern — trumi heisst schlafen — also 10 Tagereisen weit. Aehnliches berichteten HARTT und PAUL EHRENREICH von den Botokuden, berichtet VON DEN STEINEN von den Bakáiri und Kustenau in Brasilien, BEHRMANN aus Neuguinea. Der Bakaïri und der Kustenau zeigten zunächst auf die Sonne und machten dann, um möglichst klar zu sein, mit dem rechten Arm soviel Bogen am Himmel, die den Tageslauf der Sonne darstellen sollten, wie Tagereisen erforderlich waren, legten ebenso oft, um das Uebernachten anzudeuten im Anschluss an jeden Bogen die rechte Hand gegen die Wange und schlossen die Augen (10). Von den nomadisierenden Mongolen berichtet N. PRSHEWALSKI (11), dass sie grössere Entfernungen in « Pferderitten » oder « Kamelritten » angeben und von einem anderen genaueren Mass keine Idee haben. Sie fügen aber der Angabe hinzu « wenn man gut reitet » oder « wenn man langsam reitet », unterscheiden also eigentlich zwei Längenmasse. Hier ist wenigstens von einer direkten Beziehung auf den menschlichen Körper keine Rede mehr. Ob zu den genannten mongolischen Längenmassen die « Bere » oder mongolische Meile, von der POTT (12) spricht, und die acht Stimmweiten, jede von fünf hundert Klafter Länge, enthalten soll, in Beziehung steht, weiss ich nicht. Durchaus nicht in Beziehung zum menschlichen Körper scheint dann allerdings der « Kuhschrei » zu stehen, den POTT (12) als Längenmass aus Indien angibt. Wenn man allerdings hört, dass bei den in Frage kommenden Völkern als kleineres Längenmass das « Kuhohr » genommen wird, das aber gleich der Strecke von der Daumenspitze bis zur Spitze des kleinen Fingers (wohl bei aus-

(10) FETTWEIS, Das Rechnen der Naturvölker, Verlag TEUBNER, Leipzig 1927, S. 19 f.

(11) *Zeitschrift für Ethnologie*, Bd. 7 (1875) S. 365, nach PRSHEWALSKI. Die Mongolen und das Land der Tanguten, St. Petersburg 1875.

(12) POTT, Die quinare und vigesimale Zählmethode, Halle 1847, S. 2.

gespreitzter Hand?) ist, wird man wieder bedenklich, zumal POTT (12) auch angibt, der Kuhschrei enthalte acht tausend Unterarme. Ob der Hühnerschrei, den WERTHEIMER (13) nach einer persönlichen Mitteilung von Frau Prof. SELENKA als Längenmass der Dayak angibt und das Mass « so lange bis ein Kessel Wasser kocht » (13), das die Iukagiren für kleinere Strecken, die zurückgelegt werden müssen, brauchen sollen, nicht auch schliesslich noch in Beziehung zum menschlichen Körper gesetzt sind und inwieweit die Masse vom « Kuhschrei » bis zum « Kessel Wasser » überhaupt ursprünglich sind, kann ich nicht sagen.

Zum Nachweis dafür, in wie ausserordentlich engem Anschluss an die Messgelegenheit oder das Messbedürfnis ursprünglich die Messkunst entstanden sein mag, führe ich zwei Beispiele an. Das erste bieten die Lhota-Naga, ein noch ziemlich ursprüngliches Volk im nördlichen Assam (14). Diese unterscheiden nämlich ausdrücklich von ihrem Körper her *Längenmasse und Höhenmasse*. Als Längenmasse benutzen sie die Entfernung vom Brustbein bis zur Fingerspitze des ausgestreckten Arms und dann weiter die beiden Handspannen, die wir auch bei den Dajak und auf Yap fanden. Bei Höhen, z. B. bei Wasser und bei wachsendem Korn, benutzen sie aber ausdrücklich als Masse die Knöchelhöhe, die halbe und die ganze Kniehöhe, die Lendenhöhe, die Schenkelhöhe, die Taillenhöhe, die Rippenhöhe und die Brusthöhe. Grössere Entfernungen geben sie durch die Zeit an, die zum Zurücklegen gebraucht wird. Das Beispiel der Lhota-Naga widerlegt auch POTT, der im Anschluse an den Vers, OVID *Metam.* I 500 « Laudat digitosque manusque, brachiaque et lacertos » meint, von den unteren Extremitäten seien nur der Fuss bezw. dessen Bekleidung, der Schuh, und der Schritt, der der Spanne der Hand und der Klafter der Arme entspreche, aus leicht einzusehendem Grunde zum Geschäft des Messens benutzt worden. (15) Das zweite Beispiel bieten die Bewohner der Küste des nördlichen Teils der Gazellehalbinsel und der vorgelagerten Inseln, besonders der Insel Vuatam (16). Diese

(13) WERTHEIMER, Ueber das Denken der Naturvölker, *Zeitschrift für Psychologie*, I. Abteilung, Leipzig, 1912, S. 375.

(14) W. E. WITTER, Outline Grammar of the Lhota-Naga Language, Calcutta 1888.

(15) POTT, Zählmethode, S. 1.

(16) P. O. MEIER, Fischerei ..... *Anthropos*, 1913, S. 326 ff.

Leute betreiben einen ausgedehnten Fischfang. Ihre gewöhnliche Masseinheit ist die Armspanne « a pokono ». Diese benutzen sie ausgiebig bei der Konstruktion ihrer kunstvollen für den Meeresboden bestimmten Netze. Auch die aus den Stämmen umgefallener Brotfruchtbäume hergestellten Schwimmhölzer, an denen die Taue der Netze befestigt werden, misst der Eingeborene nach Armspannen. Ihre Länge ist in den verschiedenen Gegenden verschieden, von 1,5 bis 2,5 Spannen und darüber.

Die Länge der Taue aber, an denen die Netze bis zum Meeresboden hängen, misst der Eingeborene nicht mit Hilfe der Armspannen, sondern ganz anders. Er benutzt dazu die an der Küste genau abgesteckten sogenannten Uferlandstrecken. In Rakivali auf Vuatam gibt es deren drei. Die kürzeste Uferstrecke beträgt in unseren Massen gemessen etwa eintausendeinhundertfünfzig m. An ihr werden die Taue für die Küstenfischerei abgemessen. Die zweite Uferstrecke reicht ungefähr hunderd fünfzig m. weiter. Sie dient zur Abmessung der Taue für die Fischerei im sogenannten Aussenbezirk. Die dritte Uferstrecke endlich reicht noch rund zwei hundert m. weiter. Sie ist also rund tausend fünf hundert m. lang und dient zur Abmessung der Taue für die Netze der eigentlichen Hochseefischerei nördlich von Vuatam auf die Inselgruppe Neulauenburg zu. Ueberall entsprechen die Taulängen den Tiefenangaben der amtlichen Seekarte; die Eingeborenen haben die Uferstrecken deswegen jedesmal ein bestimmtes Stück länger gemacht als die von ihnen selbständig festgestellten Seetiefen, weil ja die Netze am Meeresboden einen gewissen Spielraum haben müssen. Der Einfluss fremder ausserhalb des Messbedürfnisses und der Messgelegenheit liegender Faktoren auf die Entwicklung der Messkunst zeigt sich z. B. in den Benennungen « Kuhschrei » und « Kuhohr » für gewisse Längen in Indien. Die Kuh ist nämlich den in Frage kommenden Völkern heilig. Ferner zeigt sich dieser Einfluss in den angeführten Benennungen der Yapbewohner für die beiden Handspannen. Gerade bezüglich der beiden Handspannen bei den Dajak führt Ling Roth (17) ein Erlebnis an, welches den fremden Einfluss deutlich zeigt. Er schreibt : « Once while seated in a house talking to the chief, I was a witness of a heated dispute,

(17) Ling Roth, The natives of Sarawak, II. S. 240.

which took place between two of his followers anent the sale of a pig. A pig is sold by measurement, the measurement being taken (by means of a string) of the girth of the body just behind the forelegs; and for every span's length of a string a dollar has become the fixed price. Now the buyer wanted to use the span of the second finger (18) and thumb; the seller of course objected as in a large pig the use of the longer span would materially decrease the price. After a heated discussion both parties appealed to their chief to give a decision. I was anxious to see how the old chief would get out of the difficulty, as it was evident he did not wish to offend either of them, and on the whole, I think he managed very cleverly. Both the disputants sat down in front of him and explained the point of contention, whereupon he said to the buyer : « now if you were pointing at a man (pointing at a man's eye is a form of insult) and were to do it with your second finger (18) (at the same time pointing with his second finger) how foolish it would look, would it not? ». The buyer was obliged to admit that it would be so. « Well then », said the chief, the first is the one to use and we won't adopt any new fads in this house ». The two men went away, satisfied with the chief's decision and the pig was sold. »

Wenigstens zum Teil auf die mangelhafte Entwicklung der Zähl- und Rechenkunst oder wenigstens auf deren Schwerfälligkeit möchte ich die manchmal recht grosse Zahl von nur wenig voneinander verschiedenen vom Körper hergenommenen Längenmassen zurückführen, wie sie sich z. B. bei den Lhota-Naga für die Höhen und auch auf Yap finden und auch die Tatsache, dass die Eingeborenen aus der Gegend der Gazellehalbinsel die Taulängen nicht nach Armspannen feststellen, hängt wohl zum Teil mit ihrer schwerfälligen Rechenkunst zusammen.

Düsseldorf EWALD FETTWEIS.

(18) Unter « second finger » ist hier der Mittelfinger zu verstehen.

Some Old Time Figures of the Shipholder, Echeneis or Remora, Holding the Ship

I have elsewhere (1) brought together the various legends of the shipholder and have given four explanations, of which one is purely mythical to fit the legends, and three are founded on fact. The mythical one is that the remora adhering to the ship, preferably by prow or keel, acts as a rudder, turning the ship hither or yon, or causing it to stand still.

This myth or legend was very popular during classical, mediaeval and renaissance times as is attested by the large number of accounts brought together in my paper. While writing this it struck me that this story should have been a fertile field for the illustrators of those days, but I found only one figure of it. However, by working through various old books on emblems and symbols as well as old encyclopaedias and natural histories, I have lately found a number of other figures. These are very quaint and very interesting, and it has seemed worthwhile to bring them together with some brief descriptions.

However, it will be well to give just here a figure of the remora, that the non-ichthyological reader may know how the fish looks and by what it adheres to ship, shark, turtle or other object. Fig. 1. portrays a small specimen of this fish, but one adult in all its structures. This fish differs particularly from any ordinary, rather slender fish in having on the back of the head and over the shoulders, a « Venetian blind », or rubber-shoe-sole object, made of the transformed or transmuted spinous dorsal fin. This sucker has a circumferential band or area. When this is laid on the side of a vessel, the « slats » of the « Venetian blind » lie flat. Then by muscular action they and the circumferential band are

(1) GUDGER, E. W. The Myth of the Shipholder. *Annals and Magazine of Natural History*, 9. ser. vol. 2, 271-307. 2 pls. 10 text-figs., 1918.

raised. This creates a cavity having a partial vacuum, and thus the fish is held fast. This sucker works precisely as does the wet leather sucker which boys use to lift smooth stones.

Before presenting and describing the figures which are the *raison d'être* of this article, it will be of interest to set forth three or four of these myths that the reader may know what this is and may realize how implicitly this matter was believed in various parts of the world until comparatively recent times. Of the ancients, the account given by PLINY (2) is the fullest and most circumstantial. He says that :—

« And yet all these forces [winds, tides, etc.]... a single fish, and that of a very diminutive size ... the fish known as the ' Echeneis ' ... possesses the power of counteracting ... A fish bridles the impetuous violence of the deep, and subdues the frantic rage of the universe — and all this by no effort of its own, no act of resistance on its part, no act at all, in fact, but that of adhering to the bark ...

« At the battle of Actium, it is said, a fish of this kind stopped the praetorian ship of ANTONIUS in its course, at the moment he was hastening from ship to ship to encourage and exhort his men, and so compelled him to leave it and go aboard another. Hence it was, that the fleet of CAESAR gained the advantage in the onset, and charged with redoubled impetuousity. In our own time too, one of these fish arrested the ship of the Emperor CAIUS (CALIGULA) in its own course when he was returning from Astura to Antium : and thus, as the result proved, did an insignificant fish give presage of great events; for no sooner had the emperor returned to Rome than he was pierced by the weapons of his own soldiers. Nor did this sudden stoppage of the ship long remain a mystery; the cause being perceived upon finding that, out of the whole fleet, the emperor's five-banked galley was the only one that was making no way. The moment this was discovered some of the sailors plunged into the sea, and on making a search about the ship's sides, they found an Echeneis adhering to the rudder. Upon its being shown to the emperor, he strongly expressed his indignation that such an obstacle as this should have impeded his progress, and have rendered powerless the hearty endeavours of some four hundred men. One thing too, it is well known, more particularly surprised him, how it was possible that the fish, while adhering to the ship, should arrest its progress, and yet have no such power when brought on board ».

(2) PLINY THE SECOND. « Natural History », Book XXXII, Chap. 1. Translated by BOSTOCK and RILEY, London, 1855-57.

More poetic is the brief account from AELIAN (3) :—

« Echeneis is a pelagic fish, black in appearance, equal in length to an average-sized eel, and named for the thing it does. For adhering with its teeth to the extreme stern of a ship driven by a following wind and full sails, just as an unmastered and unbridled horse is held in with a strong rein, so the fish overcomes the most violent onset of the winds and holds the ship as if tied fast to her wharf. In vain the middle sails belly out, in vain the winds rush forth, it holds steady the thing to which it adheres. The sailors know this indeed for the cause of this matter. Hence the name given to this fish, which, because of their experience with it, they call Echeneida (Remora). »

About 1250 BARTHOLOMÆUS ANGLICUS wrote his encyclopaedic « De Proprietatibus Rerum », which was so popular that it passed through 26 *dated* editions between 1470 and 1495. The first English version was printed by WYNKEN DE WORDE at Westminster in 1494 or 1495. This, while not illustrated, contains the first account in English of the ship-stopper legend. And since a copy of this priceless work is to be found in the New York Public Library, I am able to reproduce as Fig. 2. herein a photograph of BARTHOLOMEW's (4) actual text.

Even as late as 1778, this power of the remora was believed in by the fishermen of the Adriatic Sea as the following quotation from the « Travels in Dalmatia » of the Abbé ALBERTO FORTIS shows.

« We were sailing in a small bark between Vruillia and Almissa with a fresh equal gale, in the afternoon. The mariners were all at rest, and the steersman only was awake, and attended alone in silence to the direction of the bark; when, on a sudden, we heard him call aloud to one of his companions, ordering him to come and kill the Paklara. Our learned friend Signior GUILIO BAJAMONTI was with me, and understanding what the man meant, desired him to show him the fish that he wanted killed, but the fish was gone. Having interrogated the steersman, who did not want sense, and was a fisherman by profession, why he had ordered the Paklara killed, and what harm it had done; he answered, without hesitation, that the Paklara used to take hold of the rudder

(3) AELIANUS. « De Natura Animalium, Libri XVII. » Book I, Chap. 36; Book II, Chap. 17.

(4) BARTHOLOMÆUS ANGLICUS. " All the Proprytees of Thinges ". JOHN TREVISA, translator. Westmestre, 1494 or 1495.

with his teeth, and retarded the course of the bark so sensibly, that not only he, but every man who sat at the helm felt it there without seeing it. He added, that many a time he himself had catched the Paklara in the act and had frequently killed and eat it. »

At the time of publication of my article, the earliest and indeed the only figure known to me illustrating this legend was contained in the 1536 edition (Argentorati — Strassburg) of the « Hortus Sanitatis ». The authorship of this work has generally been ascribed to JOHANN WONNECKEN CUBE or VON CUBA, town physician of Frankfort toward the close of the 15th century. At the present time, doubt has been thrown on his authorship, but this need not concern us here. Fig. 3 is a reproduction of the illustration at the head of Chapter XXXVI in the 1536 edition. In this, which certainly leaves much to the imagination, there are seven sucking-fishes adherent to what purports to be a ship, but what much more resembles a bastion-like fortification projecting out into the sea, and having a mast or flag pole with twelve supporting guys. What the tetrapodous animals in the upper left of the figure are intended to represent, I do not know.

I have been interested to see if there are any variants of this representation in other editions of this work, which as indicated above ran through many editions and versions. The Pierpont Morgan Library in this city possesses five editions and these it has been my privilege to examine. In the first Strassburg edition bearing the assigned but somewhat problematical date, 1497, is found the illustration shown herein as Fig. 4. Here we have six (not 7) sucking-fish adherent to an undoubted vessel. On the lower right are three six-footed crustacean-like animals playing around a rock projecting above the water. This is a much better figure than the much later one (39 years) shown in Fig. 3. In the Morgan Library are also copies of the Mainz edition of 1491, the first Paris edition and version of *circa* 1500, the Venice edition of 1511, and the 1517 Strassburg edition. All have the same figure as that shown in text-figure 4, but drawn reversed right and left, i. e., a mirror image. The 1485 (Mainz) edition, and the 1515 (Strassburg) editions in the New York Public Library do not have the figure. These are all the editions and versions known to me in New York and further than these I have not been able to go.

Before presenting the next figures showing the shipholder atwork ,

it will interest the reader to know that such numbers of remoras adherent to a small vessel as shown in Figs. 3 and 4 would actually retard its progress. In my paper above referred to I have brought together testimony of seven writers on this matter. However, one only of these need be quoted. The well-known Australian ichthyologist, Mr. DAVID G. STEAD, thus writes in his « Fishes of Australia » (1906, p. 190) :—

> Now, though it would be altogether impossible and out of all reason to suppose that one individual [Echeneis] could exert sufficient power [materially] to delay or retard a vessel's progress, still an instance has actually come under my notice in which a sailing vessel was considerably delayed while in tropical seas through a shoal of « Suckers » attaching themselves all round its sides and bottom.

On this matter one of the most experienced fisherman at Key West, Florida, once gave interesting testimony to me. We were trying to catch an *Echeneis naucrates* adherent to a large shark, and I spoke of this matter of retardation of the shark's or a boat's speed by an attached sucking-fish and said that it seemed hardly possible. Then this man earnestly said :—

> They sure will hold a boat. I have seen ten or twelve under a boat at one time. This was while Kingfishing at Bahia Honda once. The Kingfish were in big schools and were followed by hundreds of sharks. The suckers on the boat came from the sharks. My brother and me had boats just like each other in size and build but his was a little better sailer than mine. The first day he beat me, sailing before the wind, but the second day I beat him. He said, « No wonder I am losing, too many suckers hanging on her bottom ». All Key West fishermen know that suckers will sure hold a boat.

That ships may actually be made to lose way and suffer retardation, but from another cause, is an actual fact, and for this there is a physical explanation — that of « dead-water ». This has been worked out experimentally by V. WILFRID EKMAN. (5)

« Dead water » appears to be due to a layer of fresh or brackish-water resting on the heavier sea-water. A vessel moving at slow speed in such « dead-water » sets up waves at the junction of the two layers which effectually limit the directive power of

(5) « On Dead Water », paper no. XV of the Norwegian North Polar Expedition, 1893-1896 — in vol. V Scientific Results edited by FRIDTJOF NANSEN. Christiana.

her rudder and retard the vessel. For accounts of this phenomenon and for a fuller explanation, see EKMAN above or my paper previously referred to.

Leaving now these old encyclopaedias, the next oldest figure known to me is found in the « Historia de Gentibus Septentrionalibus » (History of the Northern Peoples) of that stout old story-teller, OLAUS MAGNUS, « Bishop of Upsala and Primate of Suetia and Gothia ». In the first edition of his book (Romae, 1555, p. 761) he enumerates among the dangers undergone of sailors, the infestation by huge monsters (whales are probably meant), birds and very small fishes. And in his illustration, (Fig. 5 herein) we see on the right « the great beasts » to appease which the sailors are casting out the cargo of the ship; and, circling around the ship on the left are great birds which seemingly are threatening to carry off the sailors, while down below two « minimi pisciculi » have laid hold of the ship and have stopped her in her course. Surely navigation in those early days was a perilous matter, if one is to follow the good bishop.

In an edition of this same work published 12 years later (Basiliae, 1567) there is a slight variant of this figure which has been redrawn and now appears as is shown in Fig. 6 herein. Here the sea monsters are seen attacking the ship on the left, the birds are flying around the one in the center, and that on the right is held fast by two remoras attached near the bow.

The next somewhat abundant sources of the figures under consideration are the old books of emblems. Only a few of these have been searched, those in which I have had some hint that there were such figures. The first of these is from ANDREA ALCIATI's book, a collection of moral sayings in Latin verse, which were very popular. I have not seen the original of the first edition, « Emblematum Liber », (Augustanum — Augsburg?, 1531), but have examined the facsimile reprint of the Holbein Society published at Manchester, 1870. Neither copy is paged, but under the emblem headed « Prudentes vino abstinent » we have the ship with what appears to be a part of a sea snail emerging from the water at the side of the ship. Below the figure are the familiar six lines (found in all the figures in all the versions of Alciati) beginning « Parua uelut limax remora spreto impete uenti », which will be given later.

I have not thought it worth while to reproduce this figure since it is alien to the subject, but it does seem necessary to explain the presence of the snail. The variant version of the snail as the ship-delayer is found in PLINY who in turn got it from a work (no longer extant, I believe) on the natural history and geography of the East written by LICINIUS MUCIANUS, who lived during the first century A. D. This story of the shell-fish fish-stopper was widely copied for centuries, the mollusc, however, generally being designated as *Murex* instead of *Limax* as written by ALCIATI.

ALCIATI's very much admired book went through many editions and versions — at least 174 between 1531 and 1749. Of these there are twelve in the New York Public Library and in one of them « Emblematum Libellus » (Paris, 1534) on p. 53 is found the illustration reproduced herein as Fig. 7. Here we have the fish adherent to the ship by the disc on the back of its head and holding the ship steady in spite of the bellying sail. While on the land the snail *Limax* is portrayed with a sailor reaching out to touch it, Below is the six-line description and explanation which ALCIATI everywhere gives.

Another edition of ALCIATI's work, simply entitled « Emblemata » bears imprint « Patauij (Padua), 1621 ». On p. 363 is Emblema LXXXIII having the very interesting variant figure shown herein as No 8. Here the eel-like remora has with its mouth laid hold of what appears to be meant for the rudder post and is holding the ship steady against the force of the wind on the sail and the pull of four oars on each side. This figure is followed by two and one half pages of text in which many of the old legends referred to above are given in brief form.

JOANNES SAMBUCUS (born in Hungary in 1531) was a distinguished scholar and historiographer at the court of MAXIMILIAN II and his successor RUDOLPH II. In addition to his historical works, he wrote a book of emblems entitled « Emblemata cum Aliquot Nummis Antiqui Operis » published at Antwerp in 1564. On page 205 he has the illustration shown herein as Fig.9. Here the remora has laid hold of the prow or beak of the vessel with his mouth, and, although the sail is furled, all the efforts of the many rowers avail not to move it from the spot. On page 64 of this book is found the identical figure save that it has a differently sculptured frame around the picture of the ship and fish, while

above is the motto « Vel Minima Offendunt » and below is an entirely different text. In this the fish is said to be « vulgarly called Huso by the Panonians », and further along it is called « Sturiolus ». What is the meaning of this I do not know for the text is devoted to showing how large results often come from small causes. The explanatory text below the figure of the remora shown above (No. 9) reads as follows :

« Impulsam ventis, & forti remige nauem
Quis remoram dicat posse tenere mari ?
In minimis etiam vis est, mora quælibet ingens,
Ni videas, præter spem remoratur opus.
Quæ ausa est Actiacam subitò tardare carinam
Et prohibet cursus, parua echeneïs erat.
Nil igitur spernat, cui magna negotia curæ,
Qualia pisciculus semipedalis agit. »

JOACHIM CAMERARIUS (the younger) was born of a distinguished medical family in Nuremberg and became a well known physician in that city. He wrote works on medicine and in addition a book on symbols and emblems which in its completed form of four parts bore the title « Symbolorum et Emblematum ex Aquatilibus et Reptilibus Desumptorum. Centuria Quarta », imprinted « Frankfurti, 1654 ». On folio 28 *recto* of this is found the beautifully engraved illustration (apparently on copper) shown herein as Fig. 10. Here we have the remora laying hold with its mouth on to the rudder of a three-masted ship in full sail, and notwithstanding the strong wind, almost a gale, that fills the three large square sails, the remora holds the ship steady as a rock. Below the figure and on the following page are excerpts from classical writers narrating the ship-holding deeds of the remora.

Due to the fact that fishes have been much used in heraldry, I have thought that therein might be found other figures of the shipholder and the ship. But I have searched to little purpose in various works on the subject, especially in THOMAS MOULE's « Heraldry of Fish » (London, 1842), for the use of the remora as a heraldic device. A number of other books on heraldry, however, say that :— « GIOVANNI BATTISTA BOTTIGELLA of Padua who fought in the Italian wars under FERRANTI GONZAGA took for a device a ship in full sail, with the remora or sucking-fish attached,

over the motto 'Sic Frustra'. » Another crest is said to bear a remora on it with the inscription « Sic Parva Magna Cedunt » — « Thus great things yield to small ».

I have sought to find figures of these two devices that they might be reproduced herein, but all in vain. Furthermore, Mr. R. T. NICHOL of the Metropolitan Museum of Art, an expert on such matters, has very kindly looked up additional authorities but nowhere finds any reference to the use of the remora in heraldry other than these isolated ones. So it would seem that unlike other fish the remora was little, if at all, so used.

There are in the literature of Renaissance times allusions to the ship-retarding powers of the remora too numerous to mention. There may be quoted, however, the following from an unknown author :—

« The lazy remora's inhaling lips
Hung on the keel, retard the struggling ships. »

And this from RABELAIS' « Pantagruel » :—

« The sucking-fish, with secret chains
Clung to the keel, the swiftest ship detains. »

Longer and better, however, than any known to me, is the following from SPENCER's « Visions of the World's Vanity » :—

« Looking forth into the ocean wide,
A goodly ship, with banners bravely dight,
And flag in her top-gallants, I espied,
Through the main sea making her merry flight;
Fair blew the wind into her bosom right,
And th' heavens looked lovely all the while,
That she did seem to dance, as in delight,
And at her own felicity did smile;
As suddenly there clove unto her keel
A little fish, that men call Remora,
Which stopt her course, and held her by the heel
That wind nor tide could move her thence away.
Strange thing me seemeth that so small a thing
Should able be so great an one to wring. »

Furthermore in early English literature the remora is frequently referred to in allegorical fashion. Thus MAUNDRELL in his

« Aleppo to Jerusalem » (p. 46) writes « We had his promise to stay for us, but the remoras and disappointments we met with in the Road had put us backward in our journey ». Again BEN JOHNSON («Poetaster», III, 1) makes one man say to another of a third who had nearly talked him to death :

> « S' Death, I am seized on here
> By a land remora : I cannot stir,
> Nor move but as he pleases. »

Other citations of like allegorical allusions in English literature from SPENCER's day to ours might be given, but the purpose of this article is not to multiply such quotations, but rather to reproduce these old figures of the ship-holder holding the ship.

New York City E. W. GUDGER.

LEGENDS FOR SHIPHOLDER

FIG. 1. Figure of a shipholder (*Remora remora*) showing the rubber-shoe-sole or venetian-blind sucking disk on the back of the fish's head and shoulders. Photograph by E. W. Gudger.

FIG. 2. Photographic reproduction of the first account in English of the activities of the ship-stopper. From Bartholomæus Anglicus, 1494.

FIG. 3. Seven shipholders holding a ship. From the « Hortus Sanitatis », Strassburg, 1536.

FIG. 4. Six shipholders holding a ship. From the first Strassburg edition of the « Hortus Sanitatis », *c.* 1497. By courtesy of the Morgan Library.

FIG. 5. « De periculis Nautarum, infestatione maximarum beluarum, auium, & minimorum pisciculorum ». After Olaus Magnus, Rome, 1555.

FIG. 6. A variant of figure 5 illustrating the perils which attend navigation. After Olaus Magnus, Basle, 1567.

FIG. 7. The remora as the ship-delayer, with the snail also alleged to be a ship-stopper. After Alciati, Paris, 1534.

FIG. 8. Echeneis, here an eel-like creature, has laid hold of the rudder-post and thus holds the ship against the force of wind and rowers. After Alciati, Padua, 1621.

FIG. 9. Echeneis has caught the beak of the galley in his mouth and holds it fast despite the pull of thirty-three pairs of oars. After Sambucus, Antwerp, 1564.

FIG. 10. The remora having laid hold of the rudder of this three-masted ship retards its progress even though its three large sails are distended by the gale. After Camerarius, Frankfort, 1654.

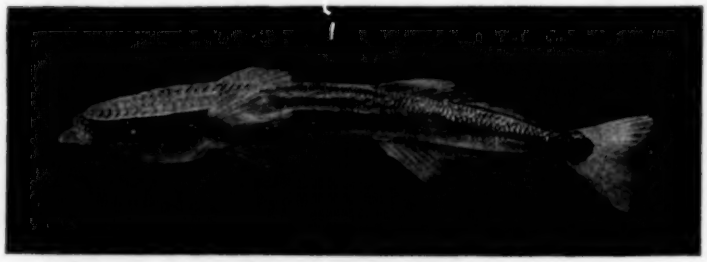
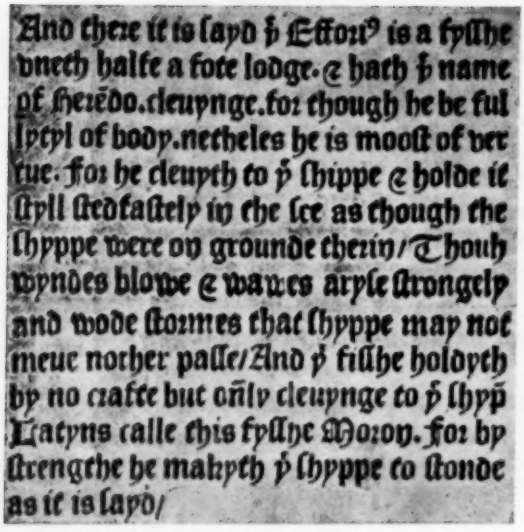


Fig. 1



And there it is layd þ Echon⁹ is a fyſſhe vneth halfe a fote lodge. & hath þ name of herēdo. cleupnge. for though he be ful lytyl of body. netheles he is moost of ver tue. for he cleupth to þ ſhippe & holde it ſtyll ſtedfaſtely in the ſee as though the ſhyppe were on grounde therin/ Thouh wyndes blowe & wawes aryſe ſtrongely and wode ſtormes that ſhyppe may not meue nother paſſe/ And þ fiſſhe holdyth by no craſte but onlv cleupnge to þ ſhyp̄ Latyns calle this fyſſhe Morou. for by ſtrengthe he makyth þ ſhyppe to ſtonde as it is layd/

Fig. 2

Fig. 3

Fig. 4

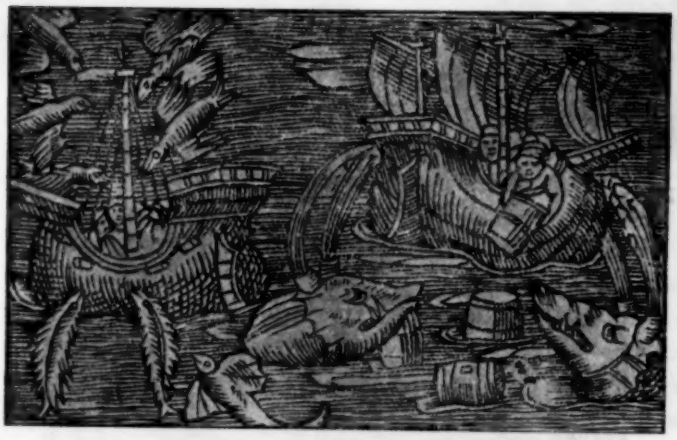


Fig. 5

Fig. 6

Fig. 8

In facile à uirtute deficientes.
Parua nelæ lunax remora ſpreto impete uenti,
Remo tamq; ratem ſiſtere ſola poteſt.
Sic quoſdam ingenio & uirtute ad ſidera uectos
Detinet in medio tramite cauſa leuis.
Anxia lis ſeluti eſt, uel qui meretricius ardor
Egregijs iuuenes ſeuocat à ſtudijs.

Fig. 7

Fig. 10

Fig. 9

Medallic Illustrations of the History of Science

(First Series : XIXth and XXth centuries)

Seventh Article.

This collection of scientific medals began to appear in volume 8 of *Isis*. The sixth article appeared in volume XII, p. 320-322, pl. 12-15, 1929. The Editor will welcome photographs or suggestions for the folowing parts of this series. It is intended to publish only medals having a sufficient scientific and artistic interest. Medals dedicated to scientists who are still living can not be published at once but will be considered for ulterior publication. GEORGE SARTON.

No. 35. P. C. JULES JANSSEN and J. NORMAN LOCKYER.

PIERRE JULES CÉSAR JANSSEN. French astronomer, Born Paris 1824, died Meudon, Seine-et-Oise, 1907.

JOSEPH NORMAN LOCKYER. English astronomer. Born Rugby, 1836; died Sidmouth, 1920.

They discovered independently in 1868 a method of spectroscopica observation of the solar chromosphere and prominences in ordinary daylight, without having to wait for an eclipse. This simultaneous discovery was celebrated by this medal published by the French government in 1872.

Artist : ALPHÉE DUBOIS. French medallist (1831-1905).

Date : 1872.

Size : 47 mm.

Collection : Cabinet des Médailles, Paris. (Courtesy of Mme. H. METZGER).

Obv. : Two portraits in profile (fig. 49). Inscribed P. C. JULES JANSSEN. J. NORMAN LOCKYER.

Rev. : Analyse des protubérances solaires, 18 août 1868 (fig. 50).

No. 36. HERMANN VON HELMHOLTZ. German physiologist. Born Potsdam, 1821; died Berlin, 1894.

Artist : RUDOLF MAYER, Austrian medallist (FORRER, 3, 630-38, 1907).

Date : 1907.

Size : 60 mm.

Collection : L. J. HENDERSON, Cambridge, Mass.

Obv. : Portrait (fig. 51). HERMANN VON HELMHOLTZ, 1858-1871. R. M.

Rev. : Den Teilnehmern am 7. internat. Physiologenkongress zu Heidelberg gewidmet von Grossherzog FRIEDRICH VON BADEN, 1907.

No. 37. ALBERT I. OF MONACO. ALBERT (HONORÉ, CHARLES) GRIMALDI. Born Paris 1848; died Paris 1922. Ruling Prince of Monaco from 1889 to his death. Promoter of oceanographic and palaeoanthropologic investigations. Oceanographer.

Artist : OSCAR ROTY (1846-1911).

Date : 1901.

Size : 35 mm.

Collection : American Numismatic Society, New York. Courtesy of HOWLAND WOOD, Curator.

This is a hundred franc gold coin.

Obv. : Portrait (fig. 52). ALBERT I. Prince de Monaco.

Rev. : (fig. 53). Deo juvante. 1901. Cent francs.

No. 38. CLAUDE BERNARD. French physiologist. Born at Saint Julien, Rhône, 1813; died Paris, 1878. Digestion of sugars and fats. Glycogenic function. Physiology of the sympathetic system. Physiological action of poisons. Introduction à l'étude de la médecine expérimentale (1865).

Artist : ALFRED BORREL. French medallist. Born, Paris, 1836. (FORRER, I, 219-22, 1904; 7, 98-100, 1923).

Date : 1879.

Size : 68 mm.

Collection : L. J. HENDERSON, Cambridge, Mass.

Obv. : Portrait (fig. 54). CLAUDE BERNARD. A. BORREL, 1879.

Rev. : Né à St. Julien (Rhône), le XII juillet 1813, mort à Paris le X février 1878. Membre de l'Académie française, de l'Académie

N° 35. FIG. 49 AND 50
P. C. JULES JANSSEN and J. NORMAN LOCKYER

N° 36. FIG. 51. H. VON HELMHOLTZ

N° 37. FIG. 52 and 53. ALBERT I OF MONACO

N° 38. FIG. 54. CLAUDE BERNARD

N° 39. FIG. 55. PAUL BERT

N° 40. FIG. 56 and 57. M. BERTHELOT

UNIV. OF MICH.

des sciences. Professeur au Collège de France, au Muséum. Commandeur de la Légion d'honneur (Laurel wreath).

No. 39. PAUL BERT. French physiologist. Born at Auxerre, Yonne, 1833; died at Hanoi, Tonkin, 1886. CLAUDE BERNARD's main disciple. La pression barométrique, 1878; etc.

Artist: ALFRED BORREL. French medallist. Born, Paris, 1836. (FORRER, I, 219-22, 1904; 7, 98-99, 1923).

Date: 1889.

Size: 68 mm.

Collection: L. J. HENDERSON, Cambridge, Mass.

Obv.: Portrait (fig. 55). PAUL BERT.

Rev.: Né à Auxerre (Yonne), le 19 oct. 1833. Professeur à la Sorbonne. Député. Ministre de l'instruction publique et des cultes. Membre de l'Institut. Résident général en Annam et au Tonkin. Mort à Hanoi le 11 nov. 1886.

No. 40. (PIERRE EUGÈNE) MARCELIN BERTHELOT. French chemist, historian of chemistry, philosopher. Born, Paris, 1827; died, Paris, 1907. Organic synthesis, thermochemistry; explosives; history of chemistry. (*Isis*, 10, 71-73, 1928).

Artist: LÉON (JULIEN) DESCHAMPS. French sculptor and medallist, born in Paris. (FORRER, I, 566-68, 1904; 7, 217, 1923).

Date: 1907.

Size: 63 × 44 mm.

Collection: Cabinet des Médailles, Paris. (Coutesy of Mme. H. METZGER).

Obv.: Portrait (fig. 56). MARCELIN BERTHELOT, 1827-1907. LÉON DESCHAMPS.

Rev.: (fig. 57) Obsèques nationales de BERTHELOT, 1907.

Notes and Correspondence

I. — The History of Science: Teaching and Personalia. Edited by HARRY ELMER BARNES (Smith College, Northampton, Mass.).

Note. The Editor of this section will be glad to receive items of information which will make the material more adequate and up to date.

1. **New York City. American Society of Medical History.** — One of the most active centers of interest in the history of science in the United States is the *American Society of Medical History* which meets periodically at the office and home of the Secretary, Dr. VICTOR ROBINSON, at 12 Mount Morris Park, West, New York City. Its honorary president is Dr. JOHN C. HEMMETER of Baltimore, and its active president, Dr. LOUIS FAUGERES BISHOP of New York City. Its sessions are devoted to papers on various aspects of the history of medicine, topical and biographical. The official journal of the Society is *Medical Life*, in which are published most of the papers presented at the meetings. The programs of a dozen representative meetings follow:

I. Dr. IAGO GALDSTON: Folk Lore in Tuberculosis. *
II. Dr. HERMAN T. RADIN: Sketch of VAN HELMONT. *
III. Endocrinology Evening. *
IV. Dr. HERMAN GOODMAN: Historical Aspects of Paresis.
Dr. VICTOR ROBINSON: Goiter Chronology. *
V. Dr. MAX KAHN: History of Acidosis. *
Dr. FENTON B. TURCK: Historical Aspects of Cytology.
VI. Dr. IAGO GALDSTON: CARL LUDWIG SCHLEICH. *
VII. Radium Evening. *
VIII. Dr. MAX KAHN and RAY G. KAHN: Quixotic Medicine. *
IX. Dr. JOHN C. HEMMETER: The Philosophy of History.
X. Dr. ABRAHAM LEVINSON: History of Modern Infant Feeding. *
XI. Balneology Evening. *
XII. Dr. MORRIS H. KAHN: History of Bronchial Asthma. *

In addition to publishing the papers read at the meetings of the American Society of Medical History, *Medical Life* prints many original contributions to medical history and related subjects. In particular, it has been pu-

* Indicates papers that have been published in the official journal of the society.

blishing of late the papers read at the joint meeting of the *History of Science Society*, the *New York Academy of Medicine* and Sections L and N of the *American Association for the Advancement of Science*, held in the auditorium of the New York Academy of Medicine on December 28th and 29th, 1928.

The moving figure in the *American Society of Medical History* is its Secretary and the Editor of *Medical Life*, Dr. VICTOR ROBINSON, whose enthusiastic editorial labors in behalf of the history of medicine have been very inadequately recognized. In addition to publishing *Medical Life*, Dr. ROBINSON has brought out a number of important books in the field of the history of medicine, chief among which have been KARL SUDHOFF's *Essays in the History of Medicine* and JOHN C. HEMMETER's *Master Minds in Medicine*. He is now engaged upon the arduous task of translating and editing KARL SUDHOFF's classic work, the famous *Handbuch der Geschichte der Medizin*.

2. **The American Association of the History of Medicine.** — The *American Association of the History of Medicine* was formed early in the year 1929, and its first meeting was held at Atlantic City in May, 1929. Dr. E. B. KRUMBHAAR of the University of Pennsylvania School of Medicine is the Secretary of the Association.

3. **The International Association of the History of Medicine.** — The next Congress of the *International Association of the History of Medicine* will be held in Rome, Italy, during the first week in September 1930. Communications should be addressed to Dr. PIETRO CAPPARONI, Via Calandrelli 10, Rome, Italy.

4. **University of California.** — A course upon the History of Biology is being offered under the auspices of the Department od Zoology. It includes a series of sixteen lectures and discussions with a short thesis to be given during one semester. The lectures are being given by Dr. C. A. KOFOID, Professor of Zoology, Dr. C. D. LEAKE, Professor of Pharmacology, and Dean LANGLEY PORTER, Dean of the Medical School.

Under the influence of a stimulating visit from the well-known bibliographers, Dr. and Mrs. LEROY CRUMMER of Omaha, an informal group has been organized to conduct a graduate seminar on the History of Medicine. The seminar may serve as a nucleus, through the influence of Professor FLORIAN CAJORI, of other groups for the study of some of the historical aspects of other sciences.

Dr. C. D. LEAKE is offering an elective course in the medical school on the History of Medicine.

The *California Medical History Seminar* was organized January 30, 1929 with a display of rare books arranged by Dr. HERBERT EVANS and

Dr. and Mrs. LEROY CRUMMER. On April 1, 1929, Prof. C. D. LEAKE presented a paper on « Roman Architectural Hygiene ». At the summer meeting Miss SONOMA COOPER presented a paper on « The Medical School of Montpellier in the Fourteenth Century ».

5. **New York University.** — Professor P. B. MCDONALD gives a course in the history of science for engineering students in New York University. In his recent book, *English and Science* (VAN NOSTRAND) he devotes the last three chapters to the analysis and recommendation of such a course as a valuable aid in broadening the specialized mind.

6. **University of Michigan.**— Professor ALEXANDER ZIWET, Professor Emeritus of Mathematics in the College of Engineering of the University of Michigan, died on November 18, 1928. He was born in Breslau, Germany, on February 8, 1853. He was noted not only as a mathematician but also as a linguist. He accumulated a considerable library, which he presented to the University of Michigan. He always maintained a keen interest in the history of mathematics and mechanics, and his library included some four hundred rare volumes dealing with these subject.

7. **Carnegie Institution.** — Dr. ALEXANDER POGO, of the Yerkes Observatory of the University of Chicago, has been appointed Fellow in the History of Science by the Carnegie Institution of Washington. He is working in the Harvard Library, Cambridge, Mass., on the history of astronomy and mathematics in the sixteenth century, under the direction of Dr. G. SARTON. His work began on July 1, 1929.

ISIS

8. M. L. GUINET, the devoted managing editor of *Isis*, has been made a knight of the Légion d'honneur on October 9, 1929. Warm congratulations.

G. S.

II. — Miscellanea.

Practical chemistry in the twelfth century. Notes to R. STEELE'S edition of RASIS *de aluminibus et salibus* (*Isis*, 12, 10-46, 1929).

« Elebla » (p. 44) : il faut l'identifier avec Niebla. Voici deux textes démonstratifs : Ex AL-MAKKARI, Analectes, ed. DOZY; Leyden, 1856; t. I, 123 :

« IBN SA'ÎD raconte... qu'il y a encore d'autres mines dans différents lieux de l'Espagne; et aussi la source de laquelle jaillit le sulfate de fer (vitriol) à Niebla (Lebla) est très renommée ».

Ex EL-CAZWINI's Kosmographie (ed. WÜSTENFELD; Göttingen, 1848; t. 2, 372) :

« Lebla, ville ancienne de l'Espagne, près de Séville. Il y a une rivière... avec trois sources: l'une d'elles est la source [proprement dite] du fleuve,

laquelle est la plus abondante et dont l'eau est la plus douce; l'autre est une source d'acide sulphurique (ar. shabb)...; la troisième est une source de sulfate de fer (ar. zâdj). »

« IBN GILGIL Cordubensis » (p. 44) : il faut l'identifier avec IBN JULJUL. *Vide* SARTON, *Introduction*, 682. Les traducteurs latins du moyen âge ne connaissaient pas les voyelles des noms propres de personnes.

(*Madrid*). MIGUEL ASÍN PALACIOS.

History of Pharmacy. — As an indication of the interest taken in the history of pharmacy, reference may be had to three national organizations. The youngest of these is the Gesellschaft für Geschichte der Pharmazie, organized in Innsbruck in 1926. The first general meeting in 1927 was naturally held in the historical city of Nuernberg with its Germanic Museum that houses so many pharmaceutical treasures. The second general session was held May 2nd to 5th in Berlin and a special exhibit was arranged in the « Schloss-Museum ». The following papers were read before the scientific session :

LUDWIG WINKLER (Innsbruck) Die Entwicklung der Heilmittellehre vom 16. bis 18. Jahrhundert.

H. GELDER (Berlin) Die Beschäftigung der Apotheker mit der Geschichte der Pharmazie.

Dr. O. ZEKERT (Wien) CARL WILHELM SCHEELE.

I. G. OBERHARD (Berlin) Zur Geschichte der russischen Apotheken seit der Revolution 1917.

WALTHER ZIMMERMANN (Illenau) Volkskunde und Geschichte der Pharmazie.

GEORGE LOCKEMANN, Alte deutsche Apotheken als Vorläufer der deutschen Universitätslaboratorien.

J. A. HAEFLIGER (Basel) Stammbücher als pharmaziehistorische Quellen.

For the latest of the Veröffentlichungen, see « *Isis* », No. 42 (N. SCHNIDERSCHNITZ, Die Geschichte der Pharmazie in Steiermark, 1929).

The French society publishes the *Bulletin de la Société d'Histoire de la Pharmacie* of which sixty-three numbers have appeared in seventeen years. The last number issued in June of this year, contains on its cover pages the following table of contents :

La communauté des chirurgiens Parisiens et la seigneurie de Marigny, par M. P. M. BONDOIS.

Compte rendu de la 23e séance de la « Société d'Histoire de la Pharmacie ».

Compte rendu de la 2e séance des « Pharmaciens bibliophiles ».

Le mouvement historique :

Revues : M. BOUVET : Le baume du Commandeur (Dr. P. DELAUNAY);

Dr. ALBAREL : RABELAIS et la thérapeutique musicale (J. D.); M. GILLE : Le scorpion dans la médecine d'autrefois (Dr. P. DELAUNAY).

Les apothicaires royaux (suite), par M. BOUVET.

La Gazette (KRATY L'ARCHIVAIRE).

DUHAMEL, pharmacien-poète, par M. EDM. LECLAIR.

L'abbé ROUSSEAU et le « baume tranquille », par M. le Dr. H. DAVID.

Documents pour servir à l'histoire de la pharmacie dans l'Ouest (suite), par P. RAMBAUD.

L'invention de l'eau de Javel, par M. le Dr. P. DORVEAUX.

Prophylaxie, par M. E. H. GUITARD.

Brindilles: La vocation de Saint-Pantaleon. Pour l'honneur des veuves.

The oldest of the three organizations referred to in the introductory paragraph is the Historical Section of the American Pharmaceutical Association conceived at the fiftieth anniversary meeting in 1902. During the two sectional meetings at Rapid City the following papers were read :

« Physics in Pharmacy, Part II. » JOHN URI LLOYD, WOLFGANG OSTWALD and WALTER HALLER.

« History of SHARP & DOHME, 1845-1929 » ALFRED R. L. DOHME.

« Reminiscences of Early Pharmacy in Baltimore » DAVID M. R. CULBRETH.

« Writing Pharmaceutical Biography and History » EDWARD KREMERS.

« Tom, the Doctor's Apprentice » EDWARD KREMERS.

« Flueckigeriana, IV : LLOYD's Conference with FLUECKIGER in New York » EDWARD KREMERS.

« LEMORT's Pharmacia » EDWARD KREMERS.

« Kava Kava (Piper methysticum). Its Ceremonial Uses in the Samoan Islands » (Illustrated by lanter slides) JOHN THOMAS LLOYD.

« The Birth of the Scientific Journal » FRED B. KILMER.

« History of the Illinois Pharmaceutical Association » W. B. DAY.

« Reciprocity in Pharmaceutical Licensure » H. C. CHRISTENSEN.

« Pharmacy in West Virginia » ROY B. COOK.

« Pharmaceutical History is Being Made » ROBERT J. RUTH.

« GARTH's Dispensary » FREDERICK J. WULLING.

« Three Pioneers and Their Activities in Initiating Federal Food and Drug Legislation » LYMAN F. KEBLER.

« GEORGE THORNDIKE ANGELL, the Fearless Pioneer for Pure Food and Drugs, and the Friend of Those Who Could Not Speak for Themselves » LYMAN F. KEBLER.

« Medical and Pharmaceutical Knowledge of BALZAC » L. E. WARREN.

« GARCIA DA ORTA and the First Description of Asiatic Plant Drugs by a European » LOUIS H. RODDIS.

« A Half Century of Pharmacy in Kansas » MATHIAS NOLL.

« GMELIN, a German Family of Pharmacists, Chemists and Botanists » OTTO RAUBENHEIMER.

« Pharmacy in Classical Antiquity » LEO R. A. SUPPAN.

(*Madison, Wis.*). E. KREMERS.

Paracelsus-Gesellschaft. — PARACELSUS ist in den letzten Jahren stark in den Vordergrund gerückt und das mit Recht, denn er hat unserer Zeit, die nach neuem Inhalt drängt, noch viel zu sagen. Jahrhunderte nach seinem Tod tritt er erst heute ganz in das Bewusstsein seines Volkes ein. Wie er den geistigen Gehalt des Mittelalters noch voll besitzt, so nimmt er weit über die Renaissance hinaus an den Gedanken und Schöpfungen teil, mit denen unsere Zeit sich zu befassen angefangen hat. Selbst Kind einer Epoche der Gärung, die stürmisch und inbrünstig nach Neuem verlangte, findet er heute Gleichgesinnte fast auf allen geistigen Gebieten. Alle, die heute in der Naturerfassung eine neue Tiefe suchen, damit der ganze Mensch wieder wirklich zu Hause sei in Freiheit und gewollter Neubindung, finden die Wege dazu schon bei PARACELSUS geöffnet.

Der Name bedeutet kein historisch abgeschlossenes Programm, sondern den Ausdruck für eine Weltgesinnung aller, die noch wissen oder ahnen, dass die Wissenschaften doch im letzten Grunde untereinander verknüpft sind und dass der Mensch im Mittelpunkt dieser verknüpfenden Fäden zu stehen hat. Unausschöpfbar ist PARACELSUS wohl im ärztlichen Bezirke als der gottbegnadete Arzt, dessen Heilweg seit einem Jahrhundert von manchen wieder neu zu beschreiten mit Eifer versucht wurde. Sein Wort ist darum so eindringlich, weil er über die Bereiche seines Zeitwissens von der Natur so weit hinausgeschaut hat, weil geheimes Wissen in berückender Mannigfaltigkeit aus ihm zu strömen scheint, weil eine heisse Liebe zu den Leidenden vornehmlich aus ihm spricht, die all sein ärztlich Tun und Denken durchtränkt und ihm den Dienst am Kranken zum Gottesdienst werden lässt. Auch diesem sozialen Grundton seines Wirkens lassen sich reiche Anregungen für die heutige Zeit entnehmen.

Derart sind die Gesichtspunkte, die die Unterzeichneten veranlassen, eine Paracelsus-Gesellschaft zu gründen, die bestimmt sein soll, auf diesem geistigen Gebiet Vergangenheits- und Gegenwartsinteressen lebendig fruchtbar zu verbinden. Sie soll der weitverzweigten Paracelsusforschung einen Mittelpunkt schaffen, sie systematisch in gemeinschaftlicher Arbeit fördern und alle Freunde der Paracelsischen Gedankenwelt in gegenseitigem Austausch zusammenschliessen. Wie Art und Mannigfaltigkeit dieser Gedankenwelt sich vielfach mit den neueren Geistesbestrebungen der Gegenwart berühren, so wird ihre Richtung im ge-

schichtlichen Entwicklungsgang des abendländischen Geistes weiterwirken, wenn die gemeinschaftliche, notwendige Sinndeutung der Zeit nach neuer Lösung drängt, und so ein einheitlicher tiefer Unterstrom unserer Vergangenheit wieder an die Oberfläche steigt.

In diesem Sinne bitten wir der Paracelsus-Gesellschaft beizutreten. Zuschriften sind zu richten an die Geschäftsstelle der Paracelsus-Gesellschaft, München, Glückstrasse 8.

Aufrufende Gründungsmitglieder:

BERNHARD ASCHNER, Wien
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ERNST DARMSTÄDTER, München
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Das Ehrenpräsidium übernahmen: Herr Geheimrat Professor Dr. KARL SUDHOFF und Herr Professor Dr. FRANZ STRUNZ.

Der Vorstand setzt sich z. Z. zusammen aus den Herren: Privat-Dozent Dr. BERNHARD ASCHNER, Wien, Professor Dr. EDGAR DACQUÉ, München, Dr. ERNST DARMSTAEDTER, München, Regierungsrat HANS HOFMANN-MONTANUS, Salzburg, Geheimrat Prof. Dr. HERMANN KERSCHENSTEINER, München, Dr. RICHARD KOCH, Frankfurt a. M., Professor Dr. EDMUND v. LIPPMANN, Halle a. S., Professor Dr. EUGEN ROSENSTOCK, Breslau.

Die Geschäftsführung wurde Herrn Dr. MANFRED SCHRÖTER, München-Solln, übertragen.

Die Geschäftsstelle befindet sich München 32, Glückstrasse 8.

Beitrittskosten, M. 1. Jahresbeitrag, M. 3.

« Classics of Science » and « Classic Inventions ». — Excellent work for the popularization of the history of science and technology is being done week after week by Mrs. WATSON DAVIS in the Science News-Letter (Published by Science Service, Washington, D. C.). The aim is to give as much as possible the author's own account of his discovery or invention « in the light of his own vision and enthusiasm. »

The « Classics of Science » began to appear on September 17, 1927 and include extracts from the writings of a great number of men of science of all ages and countries (almost a hundred by the end of 1929).

The « Classic inventions » are a new departure. Their publication began on August 10, 1929, and they will appear only once every four or five weeks. They include the patents listed as basic by the U. S. Patent Office and other inventions which help form the background of the world in which we live to-day.

Thanks to Mrs. WATSON's activity, the history of science is being diffused among a much larger public than the one *Isis* can ever hope to reach, and historians of science are very thankful to her for it.

G. S.

Reviews

(Thâbit ibn Qurra). — *The Book of al-dakhîra.* Edited by G. SOBHY. XVIII + 44 pages in English; 186 + 6 pages in Arabic; 3 pl. Cairo, Government Press, 1928 (30 piastra).

The number of valuable books issuing from Cairene presses is constantly increasing. Though we made no special effort to collect them, and were less likely to know of them than of western publications, quite a few have been brought to our readers attention within recent years. Let me just recall the following. ḤASAN KAMAL: Al-ṭibb al-miṣrî al-qadîm (Ancient Egyptian medicine), Cairo, 1922 (*Isis*, 8, 198-200). AḤMAD 'ÎSÂ BEY: Al-âlât al-ṭibb (medical, surgical, and ophthalmological instruments of the Arabs), Cairo, 1925 (*Isis*, 8, 597). IBRÂHÎM RIF'AT PASHA: Mir'ât al-ḥaramain (View of the two sanctuaries), Cairo, 1925 (*Isis*, 9, 130-33). AL-JÂḤIẒ: Three essays edited by JOSHUA FINKEL, Cairo, 1926 (*Isis*, 10, 494-95). 'UBAIDALLÂH IBN BAKHTÎSHÛ': Al-rauḍa al-ṭibbiyya (The medical garden) edited by PAUL SBATH, Cairo, 1927 (*Isis*, 10, 340-49). ḤUNAIN IBN ISḤÂQ: The book of the ten treatises on the eye, edited by M. MEYERHOF, Cairo, 1928 (*Isis*, 13, 106) Four of these Arabic publications have been reviewed by M. MEYERHOF, two by myself. To these six, I must add a seventh one which is not yet completed and has not yet been reviewed in *Isis*. I am referring to the splendid Encyclopaedic dictionary of Arabic bibliography published by JOSEPH ELIAN SARKIS. It began to appear in 1928; at the time of this writing (June 1929), I have received eight parts, containing 1528 columns and reaching almost the end of the letter qâf. The last authors dealt with are those named al-QUNÂWÎ. It is a list of printed editions of Arabic writings with bibliographical notes, which is specially valuable with regard to eastern editions, many of which are but seldom seen in western countries. Of course, it is only after considerable use that one is able to determine the completeness and accuracy, i.e., the real value of such a compilation. I can only say that I am consulting it quite often, and seldom in vain. It is not very accurate with regard to western editions, but that does not worry me, as I am likely to be already acquainted with these.

Dr. G. SOBHY, professor of medicine in the Egyptian University, Cairo, has now given us a beautifully printed edition of the Kitâb al-

dhakhîra fî 'ilm al-ṭibb — which might be translated the Medical Treasure — a general medical treatise ascribed to THÂBIT IBN QURRA (second half of the ninth century; see my *Introduction*, 1, 599). This edition is based upon the unique Ms. of the Coptic Patriarchal Library of Cairo, the late CYRIL V, Patriarch of Egypt, Nubia and Abyssinia having kindly lent it to the author. It was written in Egypt, and is dated 607 (1210).

It is certainly a very old treatise, witness many particularities of style and contents, the frequent use of Syriac words, etc. The ascription to THÂBIT is quite plausible. To begin with, we know from IBN AL-QIFṬÎ and IBN ABÎ UṢAIBI'A that he wrote such a treatise, and that he wrote it for his son, SINÂN IBN THÂBIT, as is stated in the incipit. Moreover, Dr. MEYERHOF has found many quotations from it in a book of the same kind, Kitâb al-fâkhir fî-l-ṭibb, written by AL-RÂZÎ. Now AL-RÂZÎ was a younger contemporary of THÂBIT; they must have known each other in Baghdâd. AL-RÂZÎ was more than twenty years younger, and he died in 923, THÂBIT in 901.

This edition was prepared for the centenary of the hospital Qaṣr al-'ainî. The Arabic text is followed by a very brief introduction, an English summary of it (only 3 p.), and a precious glossary of technical terms (43 p.). In the introduction, Dr. SOBHY quotes a favorite medical saying of THÂBIT which is typical of the man and time :

« Nothing is more harmful to an aged person than to have a clever cook and a beautiful concubine; for the former forces him to abuse food and he falls ill, and the latter entices him to the abuse of sexual intercourse which makes him grow older still; for the comfort of the body depends on the ingestion of little amounts of food, and the comfort of the spirit is in the non-commital of sin, and the happiness of the heart is in the lack of worry, and the comfort of the tongue is in the non-abuse of talking. »

I shall not attempt to analyze the contents of the Dhakhîra, because I have good reason to hope that such an analysis will be prepared by Dr. MEYERHOF, and published in an ulterior number of *Isis*. And who could do this with greater authority? Our readers, remembering his studies on ḤUNAIN IBN ISḤÂQ (*Isis*, 8, 685-724, 1926; 13, 106-10), and on 'UBAIDALLÂH IBN BAKHTÎSHÛ' (*Isis*, 10, 340-49, 1928) will look forward to his discussion of THÂBIT's Dhakhîra.

GEORGE SARTON.

Usâmah ibn-Munqidh (1095-1188). — *An Arab-Syrian gentleman and warrior in the period of the crusades*. Memoirs of USÂMAH IBN-MUNQIDH (Kitâb al-i'tibâr). Translated from the original

manuscript by PHILIP K. HITTI. x + 265 p., 4 illus. New York, Columbia University Press, 1929 ($ 4.50).

USÂMAH's memoirs Kitâb al-i'tibâr (Learning by example) have long been known to the students of the Crusades, through HARTWIG DERENBOURG's edition and French version (Paris, 1886-93, 1895) or through the German version of GEORG SCHUMANN (Innsbruck, 1905). The present English translation, based upon the unique Escurial Ms., is nevertheless very welcome, because the author has been able to correct the text in many places thanks to his native knowledge of Syrian usage, and because he has accomplished his task with the utmost care.

USÂMAH was born in the castle of Shayzar on the Orontes (Caesarea ad Orontem), fifteen miles north of Ḥamâh, in 1095. This castle still exists in a ruined condition, and two excellent photographs of it are given in this volume. His life was spent warring against Franks, Ismâ'îlites, and others, or when there was a moment of peace, hunting lions, wild boars, etc. in Syria, 'Irâq, and Egypt. At the old age of ninety (lunar years) he completed this autobiography which is one of the best in the Arabic language. He died at Damascus in 1188.

The Memoirs are full of anecdotes concerning war experiences and the customs of the Franks (chiefly p. 161-70). For example, USÂMAH criticizes their method of ordeal by water and duel, and their inferior medical practice (see p. 162). He notes the difference between the Franks, who had become « acclimatized » to the country by long settlement and had adopted some of its manners (e.g., who abstained from eating pork) and the rough newcomers. These differences have been noted also by Latin chroniclers.

There are many medical anecdotes, concerning unusual wounds and cures, none of outstanding interest (see p. 59, 74, etc., 87, 88, 213, etc.). Among the noteworthy cures mentioned by him are : a carbuncle removed by supping a raw egg, hernia cured by eating ravens, dropsy cured by vipers in vinegar, cold cured by an Indian melon, colic cured by a dream. He also speaks of the marvelous cures made by the Christian Baghdâdite, IBN BUṬLÂN (died c. 1063; see my *Introduction*, vol. 1, 730). If my memory does not deceive me, this IBN BUṬLÂN and another Christian, THÂBIT, are the only physicians named by him.

The most valuable part of this book, however, is the abundant information relative to the chase and falconry. USÂMAH's father had been a great hunter and USÂMAH himself was not a whit inferior to his sire in that respect; he was born a sportsman and devoted to sport (meaning war and chase) seventy years of his life. These memoirs give one an altogether different impression of Arabic sport than was conveyed by

LOUIS MERCIER's *La chasse et les sports chez les Arabes* (Paris, 1927; *Isis*, 10, 511-13).

Students of falconry will find in this book considerable grist to their mill. In fact, the final part of USÂMAH's memoirs (p. 221-54) is the earliest Arabic treatise on falconry and the chase which can be assigned to a known author. USÂMAH chased with cheetahs and well knew the difference between these animals (fahd) and leopards (namir); he has much to tell about lions, wild boars, and wild asses, falcons of various kinds, etc. He describes different ways of fishing. By way of illustration, I quote the end of his book :

« One day I saw a francolin the body of which had been cut open and the crop also taken out. In the crop was a snake, about one span in length, which the francolin had eaten up. As we were once hunting, we killed a snake from the belly of which came out another snake which the first snake had swallowed whole and which was just a little smaller than the other. It is thus the nature of all animals for the strong to prey upon the weak.

Injustice is a characteristic of all souls. If thou therefore findest
Someone who does not practice it, then there must be a special reason
for it.

To encompass all the accounts of the chase, which I practiced for seventy years of my life, is neither feasible nor possible. And to lose one's time in relating fables is one of the worst calamities that may befall a person. As for me, I would seek the pardon of Allah (exalted is he !) if I were to waste the little that remains of my life in other than obedience to his will and preparation for final reward and recompense. And Allah (blessed and exalted is he !) forgives sins and lavishes bounties from his own mercy. He is the generous one who does not disappoint the man who sets his hope on him and from whom the seeker is never turned away. »

As I am finishing writing this review I hear that another English version of USÂMAH's autobiography has just been published in the Broadway Mediaeval Library. This is a very strange coincidence ! I have not seen it and the translator, GEORGE R. POTTER, is entirely unknown to me as an Arabist. The fact that he names the author Ousama ibn Mounkidh (according to the French spelling) suggests that his version is derived from DERENBOURG's French version.

PHILIP K. HITTI, born in Syria, and sometime a member of the faculty of the American University of Beirut is now professor at Princeton University. He has edited Arabic texts, and written himself an Arabic account of Syria. Nobody could be better qualified for translating and discussing the Kitâb al-i'tibâr. Let us hope that he will eventually

complete his task by giving us a critical edition of the Arabic text.

GEORGE SARTON.

E. Allison Peers. — ***Ramon Lull.*** A biography. XVIII + 454 p. London, Society for Promoting Christian Knowledge, 1929 (18 sh.).

This new life of the « Illuminated Doctor » is very welcome. The latest full-length biography, that of ANTONIO RAYMUNDO PASQUAL : ***Vindiciae lullianae, sive demonstratio critica immunitatis doctrinae*** (4 vols., Avignon, 1778; the biography proper is in vol. 1), was a century and a half old. It is true, an elaborate study by E. RENAN and B. HAURÉAU had been included in the ***Histoire littéraire de la France*** (vol. 29, 1-386, 1885), but that too had been completed or superseded in many respects by the abundant investigations of Spanish and Catalan scholars. PEERS' life is excellent, first, because he has taken full advantage of all of these investigations; second, because it is informed throughout with a spirit of deep sympathy for Beat RAMON and his native country, the beautiful island of Majorca. In this sense it may truly be said that the preparation and writing of it was a labor of love.

LULL was not a scientist, but he had invented a general method, the ***ars magna***, by means of which he believed that every conceivable problem could be solved, every thesis proved or disproved. In this he made indirectly a real contribution to science. Having realized the unescapable strength of a sound logical argument, he imagined that with some ingenuity the same logical method could be applied to the analysis and synthesis of every possible question. He was the dark forerunner of a school of logicians, whose hopes, wise or extravagant (who shall say?), reappear periodically under different forms; to be sure, he was cruder and more naïve than any other, but he was the first, and as the fundamental method of natural philosophy, the experimental method, was hardly understood in his time, it would not be fair to blame him for being unaware of it. He failed to realize that no logical argument can be stronger than the premises upon which it is built. On the contrary he fondly imagined that the logical instrument could be improved to such an extent that it would become absolutely irresistible. For example, the unbelievers would be convinced of the superiority of the Christian faith by a sort of mental jujutsu. All the sciences would be easily inculcated in the same triumphant manner. LULL's extravagance was a genuine contribution because it provided a sort of reduction to absurdity of the scholastic, or let us say of the unexperimental, point of view. His blind faith in dialectics would seem incredible, if we did not witness the occurrence of similar delusions in the minds of most propagandists

even of our own time. He was carried away by the combination of a very strong logical bent with an overpowering desire to enlighten his fellowmen.

His failure to realize that logic is limited, even as any instrument, by the very fact that it must be applied to something whose nature it cannot change, that failure was undoubtedly wrong, though more excusable in the thirteenth century than in the twentieth. But he was prophetically right in his belief that the logical method could be improved. It cannot be improved beyond its own limitations, but these were still very distant in his time, and it is possible that we ourselves have not yet reached them. We shall never be able to construct universal thinking machines, but our inventors devise every day machines which are more and more intricate, and a machine is nothing but logic materialized. We marvel at the uncanny intelligence of some machines, but perhaps the lack of intelligence of our logicians is more marvelous still.

The « ars magna » was not a « methodus imposturae » as ROGER BACON was pleased to call it, nor was it a magical tool by means of which truth would be unerringly and irresistibly established. However it does not deserve to be disposed of in the cavalier manner of the author and other critics. There is no appearance that PEERS made a genuine attempt to understand LULL's queer symbols.

The author was right in refusing to deal with the alchemical writings, for these are certainly apocryphal, as J. R. DE LUANCO proved more than fifty years ago (Barcelona, 1870). But he should have discussed a little more fully LULL's astrological views, as THORNDIKE did in his *History of magic* (vol. 2, 868, 1923).

The history of Lullism completing the biography (p. 376-400) is very well done, and there is an elaborate bibliography. The whole book is very pleasant to read; I read it from cover to cover, and every one doing so will be amply repaid for he will have in his mind a clear picture of one of the greatest personalities of the thirteenth century. LULL was not simply an extravagant logician (I selected that topic for discussion, because it disclosed the only weakness of the book), he was also one of the fathers of the Catalan language and literature, the founder of western orientalism, a hero and a saint, a spiritual brother of ST. FRANCIS, truly one of the most arresting and lovable figures of the Middle Ages.

The author is professor of Spanish in the University of Liverpool, and he has translated into English three of LULL's most important works, *The Book of the Lover and the Beloved*, *The Art of Contemplation*, *The Tree of Love*. It is clear that he is very familiar not only with Catalan literature and scholarship, but also with the holy places where LULL dreamed his wonderful dreams, chiefly Majorca and Montpellier. His

account of these places is so alluring that the reviewer was sorely tempted to drop his work and start forthwith on a pilgrimage.

GEORGE SARTON.

A. E. Clark-Kennedy. — *Stephen Hales*, D.D., F.R.S. An eighteenth century biography. XII + 256 p., 14 pl. Cambridge, University Press, 1929. New York, MACMILLAN, 1929 ($ 6.00).

I have read the biography of the great English physiologist with great pleasure. The author does not claim to have discovered many new facts but he has put together for the first time the knowledge scattered in various publications.

It is hardly necessary to insist on HALES's work on the physiology of plants. JULIUS VON SACHS paid a glowing tribute to it in his History of Botany. HALES applied Newtonian physics to the solution of physiological problems, and combined skillful experiments which made the plants speak for themselves. He was able to disprove the erroneous theory of a circulation of sap comparable to that of blood. He explained the upward flow of sap by the three factors which are still regarded the most important: transpiration, capillarity, and root pressure (1725); he was the first to measure transpiration and root pressure. His experiments in animal physiology were equally important: he measured the blood pressure, the cardiac output, the blood flow through the tissues. To be sure these measurements were imperfect, yet his was the true experimental spirit, which contains within itself the constant wish for and means of improvement. It would seem (and this was a novelty to me) that HALES had also discovered spinal reflexes in frogs and their abolition by destruction of the cord. He did not publish his discovery, but we may believe he had made it, for we have a statement to this effect from the very man to whom it is generally ascribed, ROBERT WHYTT (1757), to wit:

« The very ingenious Dr. HALES writes me, that, having many years since, tied a ligature about a frog's neck, to prevent any effusion of blood, he cut off its head, and, thirty hours after, observed the blood circulating freely in the web of the foot: the frog also at this time moved its body when stimulated: but, on thrusting a needle down through the spinal marrow, the animal was strongly convulsed, and, immediately after, became motionless. » (WHYTT's Physiological essays, Edinburgh, 1755, 176).

However HALES's fundamental investigations in animal and plant physiology are so well known that I was less interested in the clear account which is given of them, than in the chapters describing his other activities.

Before speaking of these, I would like to draw attention to a typical characteristic of this great experimental philosopher, his prudence and slowness in the publication of his results. Though he was a born physiologist, and began his investigations very early, his earliest paper—dealing with the raising of sap in trees—was not presented to the Royal Society until 1718 (he was then 41 years old). His second, elaborating the same subject, appeared seven years later. When his first book came out, the *Vegetable Statics*, he was fifty years old like the author of the *Origin of Species*. His remarkable prudence reminds one of that exhibited by HARVEY, and by his contemporary and fellow-minister, BRADLEY. The *Haemastatics* appeared only in 1733, and some of the experiments recorded in it went back to the beginning of his scientific career.

When we think of HALES, our gratitude goes out primarily to the author of the *Vegetable Statics* and the *Haemastatics*; it is thus somewhat of a shock to find that the Copley medal was awarded to him in 1739, not for these immortal works, but for his insignificant experiments on the calculus. Was it not in that very year that Mrs. JOANNA STEPHENS had been granted by a bill of parliament the enormous sum of five thousand pounds for a worthless method of dissolving the stone! By the way, in 1740, HALES published a pamphlet discussing rather timidly Mrs. STEPHENS's medicines, and that pamphlet was soon translated into French (Paris, 1742). I hasten to add, lest the reader be misled, that his best work received considerable praise from many quarters, even if the Copley medal was given to him for other, less creditable, achievements. *Vegetable Statics* was translated into French by BUFFON in 1735, and *Haemastatics*, by DE SAUVAGES in 1744; both volumes were also translated into German (1748), Dutch (1750) and Italian (1756).

After the publication of the *Vegetable Statics* (1727), and ever more so after that of the *Haemastatics* (1733), HALES became more and more interested in the applications of knowledge, in useful inventions. The author gives a good account of his invention of a kind of ventilator; an account which gave me special pleasure as I had often wished for more information on that subject. That invention was obviously in the air about 1740, for it was made independently by three men: our hero, SAMUEL SUTTON of London, and the Swedish engineer, MARTIN TRIEWALD (*Isis*, 12, 372). The author has taken special pains to elucidate the question, studying not only HALES's two books (1743, 1758), and SUTTON's (1745), but also the Admiralty records.

The invention was frightfully needed, not only in mines, but in ships and prisons. As we now know a perfect solution required mechanical means which did not become truly available until a century later. In

the meanwhile, the conditions obtaining in ships, especially in slave ships, and gaols were atrocious. The author quotes many illustrations of them which need not be repeated, and helps us to understand how the good Dr. HALES came to consider his invention as by far his most valuable contribution. On Nov. 4, 1760, that is, two months before his death (Jan. 4, 1761, Aet. 84), he wrote to BRADLEY :

« I thank God, Ventilators in Ships, and Air Trunks in Gaols and Hospitals etc., prove Salutary beyond my most sanguine expectations : And tho' these things are of greater Benefit to Navigation than the much desired Discovery of the Longitude, yet I desire no other Reward, than that greatest of all Rewards, which I enjoy, viz : the pleasure of having done something for the benefit of Mankind. »

Imperfect as they were, HALES's and SUTTON's ventilators were undoubtedly useful contrivances, real blessings. Unfortunately, they could not reach the root of the evil for the « gaol distemper » which claimed so many victims, was not due as HALES thought to the foulness of the air, it was nothing else but typhus and was mainly propagated by lice.

American scholars will be specially interested in chapters 8 and 10, dealing with the origins of the Colony of Georgia. HALES was one of the original trustees from 1732 when the charter of foundation was granted until 1753 when the Colony reverted to the Crown. The history of Georgia was different from that of the other English colonies in North America. These other colonies had been commercial enterprises or the result of religious differences; Georgia was both a philanthropic and an imperialistic enterprise. In a sermon which he preached in 1734 in the presence of his fellow trustees, HALES justified negro slavery as being in accordance with Christian teachings ! Yet, for various reasons, slavery was forbidden, if not actually suppressed, in Georgia from 1735 to 1749.

Our readers will probably remember that bishop BERKELEY published in 1745 a pamphlet entitled « Chain of philosophical reflections and inquiries concerning tar water » which started a tar water fad (*Isis*, 4, 150). HALES published two years later, a pamphlet on the same subject, in which he showed as usual considerable caution. His « Account of some experiments and observations on tar water » (1747) did not profess to solve medical questions for « This is the proper Province of the Physicians », he said, « which I am no ways qualified to meddle in. » But we would naturally expect a physiologist to feel more responsible than a philosopher !

Considering his growing bent toward technical questions, we are not surprized to hear that Dr. HALES was one of the founders of the Royal Society of Arts (1754). At its first meeting, which he duly attended, it considered a suggestion to offer two prizes, one for the discovery

of cobalt, and the other for the successful growth of madder in the United Kingdom.

This biography affords sidelights on some of HALES's contemporaries, notably WILLIAM STUKELEY, GILBERT WHITE, and JOHN WESLEY.

My only regret is that the author did not add a bibliography of HALES's writings. Many of his publications were in pamphlet form and some of these, if not all, must be exceedingly rare. It would be worthwhile to compile a complete list of them. Should the author feel inclined to repair that omission in a separate paper, I would be very glad to publish the latter in *Isis*. A chronological list of the main events of HALES's life would also have been very useful. GEORGES SARTON.

Jamieson B. Hurry. — ***Imhotep, the vizier and physician of King Zoser and afterwards the Egyptian god of medicine.*** Second and revised edition. XVI + 211 p., 26 illus. Oxford Press, HUMPHREY MILFORD, 1928 (10/6)

The first edition of this charming monograph appeared in 1926 (*Isis*, 11, 125); it included only XVI + 118 p., 13 pl. It is at once clear that the revised edition is much larger; much new matter and many fresh illustrations have been inserted.

Dr. HURRY is not a professional Egyptologist, but a disciple of Aesculapius or more correctly of Imhotep, practicing in Bournemouth, Hampshire. His work is not a contribution to Egyptology, but a very readable introduction to Egyptian medicine. If it has no other result than to make the average physician realize the importance and the tremendous antiquity of Egyptian culture, Dr. HURRY will not have taken pains in vain. People often ask me: « Why is your journal called *Isis*? » A short name was desirable (with as long a subtitle as might be necessary). Isis suggested that culture, the earliest about which we have such definite and abundant knowledge. It suggested the most glorious period of science, its dawn. Perhaps it would have been better to call it after THOTH, the reputed inventor of writing and mathematics, said to be the author of scientific and medical works, for I did not know fifteen years ago that various other journals had been called *Isis*. At any rate, the title *Isis* answered its purpose and the journal was not only named but blessed.

To return to IMHOTEP. The best way to conceive his antiquity is to realize that HIPPOCRATES, the Father of Medicine, comes exactly half way between him and us. Just think of that! Fifty centuries of medicine, HIPPOCRATES standing in the very middle, twenty-five centuries on either side of him, IMHOTEP at the beginning, and we at

the end. Truly IMHOTEP was not a beginner, nor are we the end, but that is how the whole medical evolution appears to us to-day.

I believe I have said enough to induce the reader to take up Dr. HURRY's book. But I must now add a few words of warning. He must know that Dr. HURRY's excellent story is by no means a certain one.

Let me tell first what is certain. IMHOTEP was a medical god of the Egyptians, witness innumerable statues and amulets, witness many temples where incubation was practiced almost exactly as later in Greece. Indeed IMHOTEP was the very prototype of ASKLEPIOS. There is very good reason to believe that the Greeks, who borrowed so abundantly from Egypt, borrowed its medical god together with much of its medical technique and antidotary. However IMHOTEP had not always been a god; for centuries he had been simply a hero. It was only during the thirty-sixth dynasty (the brilliant Saitic renaissance, 654-525) that his apotheosis took place.

And when did the hero appear? Until some ten years ago he was generally assigned to the New Kingdom, let us say to about the sixteenth century, but one of the Greek papyri discovered by GRENFELL and HUNT at Oxyrhynchus enabled us to trace his worship back to the time of MYCERINUS the son of CHEOPS, i.e., to the beginning of the nineteenth century (Oxyrhynchus papyri, part XI, 221, 1915). That is very well but bear in mind that the earlier dating is based upon this single document, almost three millennia younger than the event referred to. However we need not insist upon this, for it does not matter so much when the worship actually began.

Dr. HURRY's story is entirely based upon the thesis brilliantly defended by K. SETHE in 1902 (*Imhotep, der Asklepios der Aegypter*. Untersuchungen zur Geschichte und Altertumskunde Aegyptens, vol. 2, 1902) according to which IMHOTEP was identified with a famous vizier of King ZOSER, of the IIIrd Dynasty, who flourished about the beginning of the thirtieth century. Again this is very plausible, but not certain. The argument will not be clinched until we have found IMHOTEP's tomb, a discovery which is well within the range of possibility and would be extremely interesting.

IMHOTEP, the mortal one, was not only ZOSER's vizier, the « chief of all the works of the King of Upper and Lower Egypt », he was also a man of considerable learning and wisdom, the main lector and ritualist of his time, astronomer, physician, and architect. He was probably the builder of the step-pyramid of Sakkarah near Memphis (ZOSER's tomb), the earliest large stone structure known to us, the ancestor of all other pyramids. It is hardly necessary to remark that the construction

of such an immense monument implied already a high degree of technical development and social organization.

Of IMHOTEP's medical skill we know nothing, but if he was really the living hero who became legendary and immortal and was thought worthy of representing the very god of medicine, we must assume that he had obtained some measure of medical fame during his life-time.

Whether the vizier IMHOTEP was the human model of the demigod or not, we do not know with any certainty. We can only say that SETHE's theory, popularized by HURRY, is very plausible. At any rate IMHOTEP was the prototype of ASKLEPIOS, even as THOTH, of HERMES. This illustrates very fittingly the Egyptian origin of Greek medicine.

Every disciple of IMHOTEP ought to read this book. He will find the chapters on incubation (49-56, 105-111) and on the medical library of the Asklepieion at Memphis (112-119) of special interest, also the section devoted to Egyptian medicine (145-168). I shall not insist upon this, as I propose to speak at greater length of this fascinating subject when J. H. BREASTED's edition of the EDWIN SMITH Papyrus finally appears (*Isis*, 5, 447-49).

The illustrations are good but it is a pity that their origin is not more clearly indicated, also the size, material, and dynasty of the originals. It is a pleasure to see the hieroglyphs of ALLAN H. GARDINER's fount (*Isis*, 12, 395). The book is beautifully got up, yet remarkably cheap, a pleasant contrast with the fantastic prices which German publishers are now exacting. The Oxford Press deserves every encouragement for this liberality. GEORGE SARTON.

Henry George Farmer. — *A history of Arabian music to the XIIIth century.* XV + 264 p., 3 pl. London, LUZAC, 1929 (15 sh.).

The readers of *Isis* have been able to follow the author's studies on Arabian music since 1926 when his excellent paper entitled « Clues for the Arabian influence on European musical theory » (1925) was fully reviewed (*Isis*, 8, 508-11). Many other papers of his, most of them published in the *Journal of the Royal Asiatic Society*, have since been brought to their attention (see vol. 8, 794; vol. 9, 189, 500, 559; vol. 11, 419, 515).

The present volume does not deal with the theory of music which is reserved for a companion volume. The author's purpose has been rather to outline the cultural background of the development of Muslim music, to describe the musical life of each period and speak of the main musicians. The treatment is chronological, but only in a very rough manner. This is our first cause of quarrel with the author.

For example, we read on p. 201 : « The twelfth century opens brilliantly

with IBN BÂJJA in al-Andalus, and he is followed by MUḤAMMAD IBN AL-ḤADDÂD, IBN SAB'ÎN, and MUḤAMMAD IBN AḤMAD AL-RAQÛṬÎ.» The reader is thus led to believe that AL-RAQÛṬÎ lived in the twelfth century or not long afterwards. As a matter of fact he was still living in 1273. Further on, speaking of the same RAQÛṬÎ, the author remarks «He was a thirteenth century savant of Murcia, distinguished for his abilities in music, mathematics and medicine. Then the Christians took Murcia (thirteenth century).....». Mark the vagueness of these statements. To be sure we do not know the events of his life as well as we would wish, but there is far more to be said than the author did. And why say that the Christians took Murcia in the thirteenth century? Would we say that NAPOLEON was finally beaten in the nineteenth century? The province of Murcia was conquered upon the Muslims in 1243. The book is literally full of such laxities. The author has taken considerable pains to transliterate proper names exactly, and I have not found him in error in that respect. However, it would be better to vocalize RIQÛṬÎ than RAQÛṬÎ, this nisba being derived from the name of the Valle de Ricote, in the province of Murcia, where MUḤAMMAD IBN AḤMAD was probably born.

Aside from that chronological looseness which is very irritating, there are other weaknesses which suggest that the author did not digest his abundant material long enough. There are too many colorless or meaningless statements which would be allowed in a bibliographic introduction to the subject, but are out of place in a history. For example, what is the point of speaking of the biographical collections of IBN AL-QIFṬÎ and IBN ABÎ UṢAIBI'A in the history itself? Strangely enough the book ends rather abruptly (desinit in piscem) with the mention of their works. The different pagination of the first section dealing with ancient Arabia suggests that it was added at the eleventh hour when the rest of the book was already printed.

It is a pity that the author did not keep his book on the stocks a few years longer; he would have been able to give us then a real history. But imperfect as it is, it will be very useful. The three plates adorning it are very interesting. The first, representing a zitherlike instrument called shâhrûd, is taken from a twelfth century Ms. of the Kitâb al-mûsîqî of AL-FÂRÂBÎ. The second and third represent the musical notations of AL-KINDÎ and of ṢAFÎ-AL-DÎN ABD AL-MU'MIN. These notations were derived in all probability from Greek models.

G. S.

Max Meisel. — *A bibliography of American natural history. The pioneer century*, 1769-1865. The rôle played by the scientific societies;

scientific journals; natural history museums and botanic gardens; state geological and natural history surveys; federal exploring expeditions in the rise and progress of American botany, geology, mineralogy, palaeontology and zoology. Volume III. The institutions founded or organized between 1845 and 1865. Bibliography of books. Chronological tables. Index of authors and institutions. Addenda to volume I. XII + 749 p. Brooklyn, New York, The Premier Publishing Co., 1929 (Cost of vol. 3 alone, $ 7.50; of the three volumes, $ 20).

The first two volumes of this work were reviewed in *Isis* soon after their appearance. For vol. 1, 1924, see vol. 7, 552; for vol. 2, 1926, see vol. 10, 182. I then stated (and I like to state it again) the great importance of this compilation. No one will investigate this great subject — the pioneer century of American science, 1769-1865 — or any one of its innumerable ramifications without referring to the excellent guide which we owe to MEISEL's zeal and charity.

It will be recalled that vol. II contained a series of articles devoted to each of the « institutions which have contributed to the rise and progress of American natural history ». It dealt with the institutions founded between 1769 and 1844 (I gave a list of the eleven earlier ones, that is, the eighteenth century ones, in *Isis*, 10, 183). The first part of vol. 3 (p. 1-329) is a continuation of vol. 2, dealing with the institutions established from 1845 to 1865. It should be noted that by « institutions » are meant not only academies and societies, but journals, expeditions, surveys, in general every kind of scientific undertaking which was not simply a personal and temporary affair. To illustrate, I shall quote the « institutions » established in the quinquennial years.

1845 ABERT's explorations on the Upper Arkansas. ¶ *American journal of agriculture and science.* ¶ Cleveland Academy of natural science. FREMONT's explorations in Upper California.

1850 Annual of scientific discovery.
DERBY's reconnaissance of the Gulf of California and the Colorado River and expedition to Tulare Lake.
Mississippi state geological society.
Mississippi geological surveys.

1855 Iowa geological survey.
POPE's expedition along the 32d parallel for the boring of artesian wells.
WARREN's Sioux expedition.
Wisconsin natural history association, Milwaukee.

1865 *American journal of conchology.*
Nevada geological survey.

The second part of vol. 3 (p. 329-495) is a « Bibliography of books,

pamphlets, and miscellaneous articles on American natural history ». The time interval of this part is larger, for it deals not only with the period 1769-1865, but also with the preceding one, 1590-1768. The earlier period covers 20 pages; it begins with THOMAS HARIOT's Briefe and true report of the new found-land of Virginia (Francfort, 1590).

The rest of this volume contains chronological tables of the publications considered, and very elaborate indices of authors, naturalists (p. 525-683) and institutions.

The author deserves the gratitude of every student of American science for the precious tool he has given them.

GEORGE SARTON.

Henry Evelyn Bliss. — ***The organization of knowledge and the system of the sciences.*** With an introduction by JOHN DEWEY. XX + 433 p., New York, HENRY HOLT, 1929. (five dollars)

Taking this book in hand, the reader will ask himself first of all what does the author mean by « the organization of knowledge ». A complete answer is given in his chapter 4, but a briefer one is found already in the preface « The organization of knowledge comprehends not only the ***mental*** processes, the development of concepts and the conceptual synthesis of knowledge, but also the ***intellectual*** correlation and systemization of valid knowledge, from the simpler social synthesis of common experience and elementary education to the more complex conceptual systems of science and philosophy. » This reminds one of the distinction between internal and external organization and their interdependence emphasized in the original program of *Isis* and more clearly in vol. 1, p. 195. This point of view was very obvious in the earlier numbers of *Isis*, which bore the subtitle « Revue consacrée à l'histoire et à l'*organisation* de la science ». Later the abundance of purely historical materials obliged us to neglect comparatively the philosophical or methodological aspects of our subject. Moreover it was felt that it was essential to ascertain the historical facts as well as possible; there would be time enough later to philosophize about them. And there are to-day dozens of men, who are ready to philosophize about science, for each one who is willing to establish the historical facts in a humbler spirit. This tendency makes me often think of the similar one which revealed itself at the beginning of the nineteenth century with regard to older branches of knowledge. Our biologists are not so ready to write a « Philosophie Zoologique » nowadays as they were a century ago, and this change of mood is certainly due to their increased knowledge.

This is not said in criticism of BLISS's work, which is the fruit of much labor and experience and of a wellbalanced vision. To illustrate, here

is what he says of classifications : « All classifications are in truth relative to views or conceptions, interests or purposes, and are therefore in some respects arbitrary and conceptual. But with growing experience, knowledge, and education the relevant classifications become progressively rational and systematic. This developing organization of knowledge is not, however, rigidly structural and static, but functional and plastic, and it should be liberally adaptive to new interests and changing communal conceptions ». The author insists — and in this, I heartily agree with him, — that though our classifications are impermanent, they are not so absolutely. It is up to us — historians and philosophers — to devise classifications which correspond more and more adequately to the nature of things, and are thus less and less impermanent. It is conceivable that as our knowledge grows or deepens, some classifications at least may become almost permanent in terms of human duration (i.e., they may seem permanent in contrast with the very impermanence of our own lives).

The author has written his book primarily for educators and librarians, and if the latter manage to read it, and what is better, to assimilate it, it will do them a great deal of good. Thus far the education of American librarians has been far too superficial.

The book is divided as follows : 1. The organization of knowledge; 2. Classification, synthesis, and the order of nature; 3. The system of the sciences; 4. A historical survey of systems of knowledge. The historical survey might have been considerably improved, chiefly the mediaeval part of it, which is very weak indeed. Yet even that part is sufficient for the main purpose which is to make the people realize the antiquity of the problems involved, the magnificent intellectual traditions which are back of them.

I much hope that many educators and librarians will read this work. It ought to be prescribed and discussed in every library school, though even then one should not expect too much from it. Librarians will be better when they will know more not simply of the forms and classes but of the things themselves, when they will have some familiarity with at least one kind of knowledge. In the meanwhile, BLISS's book will help them to realize more clearly the intellectual needs of their profession.

G. S.

Ettore Bortolotti. — ***Studi e ricerche sulla storia della matematica in Italia nei secoli XVI e XVII.*** VIII + 246 p., 44 fig., 8 pl. Bologna, N. ZANICHELLI, 1928. (20 lire).

This book of reprints was prepared, for the International Congress of Mathematicians which met at Bologna in 1928, by E. BORTOLOTTI,

one of the chairmen of the section of Philosophy and History of Mathematics.

The subject matter of the nine reprints can be divided in two groups : algebra, especially cubic equations, and the dawn of the integral and differential calculus in the writings of CAVALIERI and TORRICELLI.

In a collection of papers by the same historian and on the same subject, a changing point of view sometimes results in a distorted perspective; the collection of his papers may become more valuable for the study of the evolution of his views than for the history of his subject. The homogeneity of BORTOLOTTI's papers on the « fortunato periodo » of Bolognese mathematics is assured by the fact that the nine papers included in this book were published between 1924 and 1928. It is to be regretted, however, that some of his earlier papers (1919-1922), e.g., on CATALDI (1518-1626) or on the first applications of the integral calculus to the determination of the center of gravity of geometrical figures, were not included—simply because the reprints were exhausted, and there was no time to have them reprinted.

The book contains the following papers :

(1) L'algebra nella scuola matematica bolognese del secolo XVI. (Reprinted from : *Periodico di Matematiche*, vol. 5, p. 147-184, 1925, 1 fig., 8 pl.)

The contributions to algebra made by SCIPIONE DI FLORIANO DI GERI DAL FERRO (b. 1465), HIERONIMO CARDANO (1501-76), LODOVICO FERRARI (1522-65), RAFFAEL BOMBELLI, and POMPEO BOLOGNETTI form the nucleus of this study. Eight plates of photographic reproductions from Bolognese manuscripts illustrate this interesting paper on the golden age of algebra at Bologna.

(2) I contributi del TARTAGLIA, del CARDANO, del FERRARI, e della scuola matematica bolognese alla teoria algebrica delle equazioni cubiche. (From : *Studi e memorie per la storia dell'università di Bologna*, vol. 9. Imola, 1926. 54 p.)

In order to prove that CARDANO, FERRARI, and BOMBELLI—and not TARTAGLIA—advanced beyond the solution of $x^3 + px + q = 0$, already known to SCIPIONE DAL FERRO, the author ascends to the sources. The material thus conveniently brought together for further studies of the famous controversy, consists of the relevant passages from CARDAN's *Ars Magna* (1545), TARTAGLIA's *Quesiti et inventioni diverse* (1546) and *General Trattato di numeri e misure* (1556-60), and FERRARI's *Cartelli di matematica disfida*, with TARTAGLIA's *Risposte* (1547-48).

(3) Il Primo fra i Quesiti proposti dal FERRARI al TARTAGLIA nel suo 3° Cartello di matematica disfida, e la inscrizione nel cerchio dei poligoni

regolari di 7 e di 9 lati. (From : *Rend. R. Accad. Scienze Istit. Bologna*, 1925-26 14 p.).

The correspondence between LODOVICO FERRARI and NICOLÒ TARTAGLIA (1547-48) has been published in 1876 by ENRICO GIORDANI, in a limited edition of 212 numbered copies. A historico-critical study of this rich source of information still remains to be made.

(4) Sulla scoperta della risoluzione algebrica delle equazioni del quarto grado. (From : *Periodico di Matematiche*, vol. 6, p. 217-230, 1926 1 fig.).

Extracts from TARTAGLIA's Quesiti form the nucleus of this interesting paper. TARTAGLIA, CARDAN, the youthful LODOVICO FERRARI, and « quel diavolo de messer ZUANE COLLE », i.e., Maestro ZUANNE DA COI, or ZUANE ANTONIO libraro, or GIOVANNI TONINI da Collio live again in their spoken or written words.

(5) Primordi della geometria analitica. L'algebra geometrica di PAOLO BONASONI nel MS 314 della Biblioteca Universitaria di Bologna. (From : *Rend. R. Accad. Scienze Istit. Bologna*, 1924-25 18 p.).

An analysis of the recently discovered Algebra geometrica inventa a PAULO BONASONO Bononiensi, written, apparently, before 1587.

(6) I progressi del metodo infinitesimale nell'opera geometrica di EVANGELISTA TORRICELLI. (From : *Periodico di Matematiche*, vol. 8, p. 19-59, 1928. 29 fig).

This paper can be considered as a continuation of the TORRICELLI studies reprinted under (7) and (8). It deals chiefly with the indivisibili of BONAVENTURA CAVALIERI (1598-1647) and of EVANGELISTA TORRICELLI (1608-47), and with TORRICELLI's treatment of ratios of infinitesimals of the same order.

(7) La scoperta e le successive generalizzazioni di un teorema fondamentale di calcolo integrale. (From : *Archivio di Storia della Scienza*, vol. 5, p. 205-227, 1924 3 fig.).

A discussion of TORRICELLI's anticipation (1646) of FERMAT's generalization (1657) of CAVALIERI's theorem (1629; Geometria Indivisibilibus continuorum nova quadam ratione promota, lib. II, prop. 19 and 24; also, problema 100 in his Centuria di vari problemi).

(8) La memoria *De infinitis hyperbolis* di TORRICELLI. (From : *Archivio di Storia della Scienza*, vol. 6, p. 49-58 and 139-152, 1925, 7 fig.).

A continuation of the preceding paper. BORTOLOTTI gives the history, analysis, and systematic exposition of the contents of TORRICELLI's *De infinitis hyperbolis*.

(9) Le prime rettificazioni di un arco di curva nella memoria : *De infinitis spiralibus* di TORRICELLI. (From : *Rend. R. Accad. Scienze Istit. Bologna*, 1927-28 14 p., 3 fig.).

GINO LORIA, in his paper *Evangelista Torricelli e la prima rettificazione*

di una curva, (*Rend. Acc. Lincei*, 1897), expressed the hope that TORRICELLI's *De infinitis spiralibus* will be, finally, published, and that the right order of the links of the chain of TORRICELLI's reasoning will be reestablished. LORIA's 1919 edition of TORRICELLI's geometrical works fulfilled the first hope. The continuity of the « catena dei ragionamenti » is reestablished by simply joining the first and last link, and then beginning to read on page 11, at « Prima definitio et vera ». BORTOLOTTI gives a historico-critical analysis of this neglected masterpiece.

The edition of BORTOLOTTI's Studi e ricerche was limited—apparently by the lowest available number of reprints—to 140 copies. The publisher, NICOLA ZANICHELLI, is responsible for the preface and for the reprints (1), (4), and (6) only, which contain their usual quota of misprints in the French and German bibliographical references.

(Cambridge, Mass.) A. POGO.

Helen M. Walker. — ***Studies in the history of statistical method.*** With special reference to certain educational problems. VIII + 229 p. 12 illus. Baltimore, WILLIAMS and WILKINS, 1929.

The appearance of this volume gave me great pleasure, for I had felt the need of it for a considerable time. The author is well known to our readers, because of her study on The relation of PLANA and BRAVAIS to theory of correlation (*Isis*, 10, 466-84, 1928).

A brief introductory chapter is followed by the one entitled « The normal curve » which includes what might be called the prehistory of the subject and deals with the first studies on probability by PASCAL, FERMAT, HUYGENS, the subject becoming gradually more precise and more complex. The works of the two great pioneers, JAMES BERNOULLI and ABRAHAM DE MOIVRE, are treated more fully. The former's *Ars conjectandi* appeared only in 1713, eight years after his death : its second centenary was celebrated by the Russian Academy in St. Petersburg, 1913 (*Isis*, 2, 395). ABRAHAM DE MOIVRE was the real discoverer of the normal curve, and the essence of this fundamental discovery was contained in the *Approximatio ad summam terminorum binomii* $(a + b)^n$ *in seriem expansi*, privately published by him on November 12, 1733. This exceedingly rare pamphlet was reproduced in facsimile in *Isis* (vol. 8, 671-84, 1926), together with an introduction by R. C. ARCHIBALD. Every reader of *Isis* may thus feel that he owns an exact equivalent of that treasure. The other protagonists pass rapidly before our eyes. That is, their contributions are discussed and good likenesses of them given. The choice of portraits is excellent : JAMES BERNOULLI, DE MOIVRE, LAPLACE, GAUSS, QUETELET, two of GALTON, and two of PEARSON. PEARSON is the main hero of the story, one of his portraits appearing

ni the frontispiece. I was glad to see that the author did not forget to refer to the « Passionate Statistician », FLORENCE NIGHTINGALE (*Isis*, 8, 186), and the more so, because so many readers have been deceived by LYTTON STRACHEY's entertaining but perfidious account of her in his Eminent Victorians (1918).

It is out of the question to analyze a book which is as condensed as this one, nor is it at all necessary. It will suffice to indicate the titles of the following chapters :

3. Moments; 4. Percentiles; 5. Correlation; 6. The theory of two factors; 7. Statistics as a subject of instruction in American universities; 8. The origin of certain technical terms used in statistics (a very useful historical glossary); Appendix; The study of college catalogues; Bibliography; Index.

My only criticism is that some chapters are a little too inchoate. Take for example, the sixth, on the theory of two factors. We are not even told what that theory is ! We are only given the skeleton of that chapter, not the chapter itself. It is as if the author had become suddenly impatient, and had tried then to get rid of her book as quickly as possible, leaving parts of it not only in an unfinished stage, but in the form of rough materials juxtaposed, not integrated. However as it is, it is already very valuable, and may be at some later time, when Dr. WALKER will have more leisure and endurance, she will herself realize the jejuneness of her book, and take the trouble of rethinking, reshaping, and rewriting it. In this, books have a great advantage over children. Once that our children are born, there is not much to be done but to let them grow, we cannot change their potentialities any more; when they are conceived, they are conceived for ever. Books can be taken back into the womb and be reborn. Perhaps someday Dr. WALKER will give us a new edition of her Studies, having all the qualities of the first, but more substance and more flesh. GEORGE SARTON.

M. A. Blokh — *Chemical biographical reference book.* Prominent chemists and scientists of the XIXth and XXth centuries who have worked in domains of science bordering on chemistry. (In Russian). Volume I. XLIV + 512 p., 44 portraits, Leningrad, Chem.-techn. Press, 1929. (15 roubles).

A specimen of premature publication. The author has spent years in carefully collecting the material—and then suddenly sent to the printer a manuscript or, to be exact, the first half of the manuscript which was far from being in an acceptable shape. Whether this sudden outburst of creative urge came from within or is the result of a pressure coming from without—the necessity of « delivering the goods »—we do not know.

The volume under review ends abruptly, at the bottom of page 512, with the first four lines of the biography of WILHELM V. MILLER. The next volume will contain the end of this biography, the end of the main alphabetical sequence, a second list of more than five hundred « additional » biographies and supplements to biographies of the main sequence, and a Russian alphabetical index for locating the various articles and fragments of articles in the two volumes. The cutting of the book in two is explained, in the preface, by the desire to make it more accessible in price; by « accessible » the author apparently means « attractive », presuming that his prospective customers have a well-developed instalment-plan mentality.

In a reference book, the arrangement of the material is of fundamental importance. The author does not call his work a bibliographical dictionary. He is right : it is not a dictionary. The choice of the Latin alphabet for the main sequence and the future supplement was decidedly a mistake. The usual way of adding, in parentheses, the original western spelling to its Russian transliteration, and of arranging a Russian dictionary according to the Russian alphabet, did not appeal to our author. He gives no Russian transliteration of the names of foreign scientists; neither does he attempt to transcribe Russian names, so as to make them recognizable to a curious western eye. He arranged the western names in the order of the Latin alphabet, and he interpolated the names of Russian scientists according to a system which leads to annoying inconsistencies. Two groups of difficulties arise. First, there are Russian names beginning with sounds differently transliterated in the various European countries. The author has tacitly accepted the German transcription, so that the biography of KHARICHKOV (the name is spelled in Russian only, as stated above; the sound of the first syllable is identical with that of the German word Charakter) is found between the optically but not acoustically appropriate neighbors CHARDONET and CHARLES, whereas Russian names beginning with the sounds equivalent to the French Ch (English Sh) and to the English Ch, will probably appear, in the second volume, where the German Sch and Tsch could be expected to be listed. The other difficulty arises when an originally European name, phonetically spelled in Russian, has to be inserted in a Latin alphabetical sequence without undergoing a re-transliteration. Thus we find, after GERHARDT, the Russian chemist G. R. GHERMAN (born in Dresden, as HERMANN), and after GERSTL we find the Russian chemist GHERMAN GHENRIKHOVICH GHESS (born in Geneva, Switzerland, as HERMANN, son of HEINRICH HESS), whereas the western chemists OTTO HERMANN, CARL FELIX HERMANN, CHRISTIAN HESS, ALBERT HESSE, and JULIUS HESSE, are listed, fifty pages away, under H. The author explains, in the preface, that his choice of the Latin alphabet was made for two

reasons : because of the interest evinced by foreign historians of science in his work, and because of the impossibility of an exact Russian transliteration of foreign names. Now, most western readers cannot use his book, and cannot even get acquainted with the names of the less well known Russian scientists, since no Latin transcription of Russian names is given. As for the difficulties of transliteration, we have seen that the author fell into Scylla, trying to avoid Charybdis, whereas the old-fashioned method of parenthetical addition of the original western spelling, used by most Russian biographical dictionaries leads to quite satisfactory results.

Not less important than the arrangement of the reference matter is its selection : whose names should appear ? what are the essentials of a short biography ? Let us first see who was admitted, and who was excluded. According to the subtitle of the book and to the preface, scientists of the XIXth and XXth centuries are eligible, provided that they are dead. As a result of this limitation (which is wise, so far as living lesser lights are concerned), the biography of PIERRE CURIE is given, while MME CURIE is not admitted. The great SVANTE ARRHENIUS and a host of lesser recently departed chemists will be necrologued in the promised supplement of the second volume. The author was more hospitable to scientists of the second half of the XVIIIth century (CAVENDISH, LAVOISIER, LE BLANC, LEBON, and others).

The drawing of the demarcation line between chemistry and the neighboring sciences is a matter of personal preferences. The reviewer was, however, mildly surprised, when he met, within the enclosure, the shadows of J. A. BRASHEAR, FOUCAULT, and several others.

The choice of the data to be included in a short biography depends, of course, on the available material, and on the editorial policy. Nevertheless, a certain degree of homogeneity can be attained. Writing for class-conscious readers, the author always carefully recorded the social stratum which produced the individual scientist. He was less consistent in the question of nationality. We do not know, whether internationally minded statisticians can find use for data on the nationality of scientists. But if such information is considered as irrelevant, it should not be put on record that JOH. AUG. ARFVEDSON (AFZELIUS) was a Swedish chemist and mineralogist, or that E. A. FOMINA-SHUKOVSKAYA was the first Russian woman to receive a doctor's degree in chemistry from the University of Geneva. Fortunately it is possible, in most cases, to deduce the nationality of a scientist from the biographical data given by the author. But not always. The names of obscure towns where some of the scientists were born give little useful information to the average reader; sometimes geographical data are completely

omitted. Take the case of HEINRICH GÖBEL. Our author gives the laconical information : 20. IV. 1818-16. XII. 1893. The bibliographical reference indicated by the author, if consulted, yields the following data : born, 1818, in Springe (Hanover, Germany); from 1849 until his death, 1893, in New York City. In the reviewer's opinion, the reference to Hanover, 1849, and New York is essential for a biographical frame without a picture. Incidentally, HENRY GOEBEL was not a chemist; he became widely known when the Beacon Vacuum Pump and Electrical Company, of Boston, and the General Electric Company could not agree, in 1893, on the question : when and by whom was the first carbon-filament incandescence lamp invented? Readers who are familiar with the aspect of EDISON's first lamp now celebrating its golden jubilee even on postage stamps, may profitably consult *The Electrical Engineer* (New York) of Feb. 11 and April 22, 1893, for the illustrated description of lamps entitled to a diamond jubilee; the Göbel lamp had the misfortune of being born before the day of the dynamo.

An elaborately classified bibliography precedes the reference book, occupying 34 pages; even here we have a main sequence and last-minute supplements.

It is fortunate that the author did not try to make the length of a biography correspond to the importance of the scientist; he thus gives us much otherwise inaccessible information on generally neglected Russian chemists—a contribution to the history of science for which he deserves credit.

The final opinion of the reviewer concerning this reference book is reserved until the second part, with its supplement and index, becomes available.

(Cambridge, Mass.) A. POGO.

Henry Fairfield Osborn. — ***From the Greeks to Darwin.*** Second edition., revised and extended to embrace recent scholarship. XVI + 398 p. New York, London, CHARLES SCRIBNER's Sons, 1929 ($ 2.50).

Our reader's attention has frequently been drawn to the pioneer work of EDGAR FAHS SMITH, THEODORE RICHARDS, and LAWRENCE J. HENDERSON, as teachers of the history of science in American colleges. Strangely enough, I never thought of another pioneer, the great palaeontologist, H. F. OSBORN. The present volume was first printed in 1894, having grown out of lectures delivered at Princeton in 1890 upon the period between BUFFON and DARWIN, and completed in a fuller course delivered in Columbia in 1893. Thus as early as 1890, Dr. OSBORN was already teaching the history of science at Princeton.

The readers of this book ought to keep that in mind and to think with

gratitude of the scientist who had already devoted so much attention forty years ago to a subject which the vast majority of professors and educators have not yet understood today. It must be read in that spirit; it is clear that to judge a pioneer work with any severity would hardly be fair.

The account is divided as follows :

1. The anticipation and interpretation of nature; 2. Among the Greeks; 3. The evolution idea among the theologians and natural philosophers; 4. The evolutionists of the eighteenth century; 5. From LAMARCK to ST. HILAIRE, GOETHE, and NAUDIN; 6. DARWIN; Retrospect.

In other words, a hundred pages are given to the Greeks, half as much to the whole period extending from the Greeks to the eighteenth century; fifty pages to the eighteenth century evolutionists; eighty to LAMARCK, GEOFFROY ST. HILAIRE, GOETHE, and NAUDIN; fifty to DARWIN. I would not grudge the space devoted to Greek thought, though it is almost one third of the whole, but as is usually the case for histories written by men who are primarily scientists, the account of the Middle Ages is utterly insufficient. Meager as it is, it does not even fulfill its own promise, for it is full of statements which are too vague to be truly useful. For example (p. 106) : « In proof of this Greek influence we find that AUGUSTINE also adopted some of the Greek notions of the spontaneous generation of life. In the Middle Ages analogous views were held by ERIGENA, ROSCELLINUS, WILLIAM OF OCCAM, ALBERTUS MAGNUS; and AUGUSTINE was finally followed by AQUINAS, who is now one of the leading authorities of the Church, and by ROGER BACON. BRUNO struck out into an altogether different vein of thought. » The discussion of eighteenth and nineteenth century ideas is far more instructive, and repays careful reading.

One would like to know who was responsible for the index; certainly not the author. It is fantastic; see the items « citations », « volumes ». LEONARDO DA VINCI is classified under « Da Vinci »; it is almost as bad as if he had been put under « of Vinci ». Etc.

The author of this book is too well known in America, or among biologists and palaeontologists all over the world to need any presentation, but for the sake of those readers who are neither Americans nor palaeontologists it may briefly be said that he is one of the masters of vertebrate palaeontology and the main organizer of the American Museum of Natural History in New York. Thanks to his bold leadership, this institution has become in an unbelievably short space of time, one of the greatest museums of the world. When the time comes to celebrate his memory—but let us hope it is still far distant—it will almost suffice to put a tablet in the main hall of the Museum (as was done for Sir CHRIS-

TOPHER WREN in St. Paul's) with the inscription « Si monumentum requiris : circumspice ». GEORGE SARTON.

F. S. Bodenheimer. — *Materialien zur Geschichte der Entomologie bis Linné.* Band I, x + 498 p., 155 fig., 24 pl. Berlin, JUNK, 1928. (To be completed in a second vol.; cost of both volumes — Rm. 132 bound, Rm. 120 unbound).

This history of entomology prepared by a careful scholar is exceedingly welcome. Our knowledge of the history of zoology or biology is still very imperfect. Not only is it full of gaps, but we lack critical studies of the most fundamental works, except a very few. It would not be too much to say that it has still to be worked out almost from the bottom up. A great step forwards has been made by Dr. BODENHEIMER with regard to one of the major subdivisions of the animal kingdom. Thanks to his devotion, entomology is now lifted up to a much higher level of historical consciousness than any other branch of zoology.

In order that our readers may better appreciate the importance of this work, it is well to recall briefly the main tools available today to the historian of zoology.

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(1) VICTOR CARUS : Geschichte der Zoologie (Munich, 1872).

(2) L. C. MIALL : The early naturalists. Their lives and work, 1530-1789 (London, 1912). A modest but honest and valuable book not quoted by the author.

(3) RUDOLF BURCKHARDT : Geschichte der Zoologie (Sammlung Göschen, 1907). New edition by HUBERT ERHARD (ibidem, 2 vols., 1921).

(4) WILLIAM A. LOCY : Biology and its makers (New York, 1908). Third edition revised, 1915.

(5) EMANUEL RÁDL : Geschichte der biologischen Theorien (2 vols., Leipzig, 1905-1909). Second edition of vol. 1 in 1913.

(6) ERIK NORDENSKIÖLD : Biologins Historia (3 vols., Stockholm, 1920-24). English translation (New York, 1928), *Isis*, 12, 336-40.

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(7) MAURICE BOUBIER : L'évolution de l'ornithologie (Paris, 1925). *Isis*, 8, 515-517.

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(8) HERMANN AUGUST HAGEN : Bibliotheca entomologica (2 vols., Leipzig, 1862-63).

(9) BASHFORD DEAN, EUGENE WILLIS GUDGER, etc. : A bibliography of fishes (3 vols., New York, 1916-23). *Isis*, 6, 456-59.

(10) JOHN TODD ZIMMER : Catalogue of the EDWARD E. AYER ornithological Library (2 vols., Chicago, 1926). *Isis*, 10, 94.

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(11) *Zoologische Annalen*. Edited by MAX BRAUN (7 vols., Würzburg, 1905-19).

To return to BODENHEIMER, his *Materialien* are not simply the first history of entomology on a sufficiently large scale, but the first history of any branch of zoology, with the partial exception of BOUBIER's history of ornithology above-mentioned. It is the first attempt to make an original investigation of all the materials which record the slow progress of our knowledge of the immense and mysterious world of insects. His work is less a history in the ordinary sense, than a collection of materials for such a history, the author's method being essentially to offer German versions of (or extracts from) the entomological works or items, together with brief biographical and bibliographical notes. This is excellent as far it goes, and it will have one result, the educational value of it cannot be overestimated, to wit, that entomologists, naturally endowed with some historical sense, will be taught to examine original documents, or at least to approach them as closely as possible.

The first volume of *Materialien* tells the story down to the middle of the eighteenth century. The second will contain the footnotes relative to both volumes, which are thus inseparable.

Main contents. — Introduction (10 p.). A. Oriental origins (36 p.). B. European antiquity (68 p.). C. Middle Ages (132 p.). D. Modern times (i.e., from ALDROVANDI on) (251 p.). This subdivision seems good. It is very gratifying that the author has tried to do justice to the efforts of mediaeval naturalists, instead of disposing of them as summarily as most scientists do.

Our main general criticism refers to section A, absurdly labeled « Die orientalische Urzeit », according to a childish conception shared by most historians of science. They seem to believe that science began in Asia (Ex oriente lux) but that it never grew up there. Of course this is absolutely at variance with all that we know. The earliest scientific documents come from Egypt and Mesopotamia, it is true; but nothing but unsubstantiated legends from China. Egyptian and Mesopotamian science stopped at a relatively early time, for the simple reason that these civilizations were replaced by others, but Chinese science which began—as far as we know—much later, grew very slowly during a period of more than two millennia. To speak of Chinese or Oriental science as original (uralt) and anterior to Western science is as foolish as if one spoke of insects as entirely anterior to us. In fact, it is more foolish, for the insects

are considerably older than we, even if many of them continue to live with us for our constant annoyance or benefit. We are not so sure that Chinese science is older than European science; it may be or not; at any rate it is largely coextensive with it.

BODENHEIMER's account of Chinese entomology is largely derived from the *Illustrations of husbandry and weaving* (Kêng chih t'u)—a collection of drawings made by LOU SHOU about the middle of the twelfth century. Of course much of this may represent ancient, « uralt », knowledge, but we have no right to assume it without further proof. It is a document of the twelfth century, a witness of that time; even as a treatise of 1929 is a witness of scientific knowledge in 1929, though it contains an accumulation of knowledge of all ages, from the earliest to the latest.

It is a pity that the author did not know of the Chinese extraordinary cricket lore recently revealed by BERTHOLD LAUFER (Chicago, 1927; *Isis*, 10, 510-11). The earliest Chinese treatise on crickets, the *Ts'u chih ching*, was composed by CHIA SSU-TAO, in the first half of the thirteenth century. The Chinese had developed elaborate rules with regard to the rearing and training of crickets and their medical care; they knew that the chirping is produced by the motion of the wings; they had made extensive comparisons between different kinds of crickets, and, e.g., asserted that for only one kind *(Homoeogryllus japonicus)* was it necessary to keep the female in attendance in order to induce the male chirping.

To illustrate the long continuation of Chinese culture, as opposed to the naive conception of an early culture antecedent to European antiquity, I may refer to another study of LAUFER's, his *Sino-Iranica* (Chicago, 1919; *Isis*, 3, 299-302), wherein he has shown that the Chinese importations of plants from Central Asia extended over a period of fifteen centuries.

Another illustration is provided by the slow development of the pên ts'ao. The statement (on p. 22) that « Das klassische Werk der chinesischen Drogenkunde is der Pên ts'ao » is as meaningless as if one said that the main occidental work is the herbal or the pharmacopoeia. The Pên ts'ao is not a book but a class of books whose development is coextensive with that of Chinese civilization.

Another part of BODENHEIMER's « Urzeit » deals with Jewish entomology as exemplified in the Bible and Talmud! To witness a Palestinian historian treating the Talmud as a sort of prehistoric work is rather funny. To be sure Dr. BODENHEIMER is well aware of the length and chronological position of the Talmudic period, and of the fact that much Talmudic knowledge is indirectly of Greek origin. The fact remains—and I cannot forgive it—that the Talmud is placed by him together with a number of Chinese mediaeval books in the « Urzeit ». There

is no justification whatever for such an absurdity, except the inertia of stupid custom.

But let us get out of that chaotic « Urzeit » ! The chapter on Greek and Roman entomology calls for no special remark. It contains a long section on Roman apiculture and a shorter one (too short) on sericulture and seritexture.

The mediaeval part has been generously treated.

(1) *Early middle ages* (13 p.) — Physiologus; ISIDORE OF SEVILLE; Carolingian renaissance; eleventh century; HILDEGARD; first Aristotelian renaissance.

(2) *Arabic period* (40 p.) — Translators, zoologists; entomology in the botanical and medical literature; in the agricultural literature; in the geographical and cosmographic literature. This contains long extracts from AL-DAMĪRĪ, ABŪ MANṢŪR MUWAFFAK, IBN AL-'AWWĀM, AL-QAZWĪNĪ, etc. I am quoting these names in the order in which they occur, which is quite different from the chronological order. Indeed, BODENHEMER's way of subdividing his subject breaks the chronological order repeatedly, and hence it is not at all easy to determine the gradual progress of entomology by means of his book. To do so, it would be necessary first to replace all these materials in their true sequence.

(3) *Scholastic middle ages* (37 p.) — This is a bad title; one can guess the author's meaning, but it is wrongly expressed. CANTIMPRÉ; ALBERT THE GREAT; from VINCENT OF BEAUVAIS to PETRUS CANDIDUS DECEMBER; PETRUS DE CRESCENZI, apiculture (19 p.).

(4) *Period of transition* (42 p.) — Theologico-biblical entomology; medical entomology; OLAUS MAGNUS; EDUARD WOTTON; insect trials and insect exorcisms; vermin.

The last two sections are of considerable folkloric interest, as the following list (p. 239) will show.

« Die wichtigsten Etappen des Insektenprozesses und der Insektenexkommunikation.

1121. Anathem über Mücken durch den heil. Bernhard in Foigny (Laon).

1320. Prozess gegen Maikäfer vor dem geistlichen Gericht von Avignon. Dies ist wohl der älteste überlieferte Prozess.

1338. Bozen : Verweisung der Heuschrecken von offener Kanzel durch den Pfarrer.

1497. Maikäfer-Prozess vor dem geistlichen Gericht in Lausanne.

1516. Troyes : Prozess gegen Raupen.

1534. Exkommunikation und Exorcismus von Tieren in Portugal verboten.

ca. 1550. Heuschrecken-Prozess zu Arles.

1579. Nach einem bekannten Insekten-Prozess werden Maikäfer durch den Bischof von Lausanne bei Strafe der Exkommunikation verwiesen, ' was aber nicht viel geholfen haben soll. '

1585. Raupen-Prozess vor dem geistlichen Gericht zu Valence.

1587. Chaerocampa-Prozess in Savoyen.

1602. Aldrovandi rät bei Insekten-Plagen zu kirchlichen Exorcismen.
1690-1717. Kirchliches Verbot der kirchlichen Tier-Exkommunikation, nur noch Adjuration und Besprengen mit Weihwasser erlaubt.
1733. Der letzte Tier-Prozess zu Boumanton vor kirchlichem Forum.
ca. 1830. Der letzte weltliche Tierprozess in Dänemark (in Europa). »

With regard to the final part it will suffice to give contents briefly:

(1) *Second Aristotelian renaissance and foundation of modern taxonomy and morphology*. — Beginnings of personal observation (this subtitle is misleading): ULYSSES ALDROVANDI; THOMAS MOUFFET; JOHN JOHNSTON, WALTER CHARLETON, and JOHANN SPERLING; other local faunas, CASPAR SCHWENCKFELD, JOH. BAUHIN, LEONHARD BALDNER.

(2) *The bionomic period* (1660-1750): (a) The biologic renaissance, FRANCIS BACON, RENÉ DESCARTES, WILLIAM HARVEY, G. A. BORELLI, FRANCESCO REDI, ATHANASIUS KIRCHER. (b) Beginnings of microscopy. FRANCESCO STELLUTI, FEDERICO ANGELO CESI, MARCELLO MALPIGHI, JAN SWAMMERDAM, ANTONY VAN LEEUWENHOEK, ROBERT HOOKE. (c) The great bionomists. JAN GOEDART, MARIA SIBYLLA MERIAN, ANTONIO VALLISNIERI, RÉAUMUR, J. L. FRISCH, A. J. ROESEL VON ROSENHOF, CHARLES BONNET, JOHN RAY, ELEAZAR ALBIN, WILLIAM GOULD.

A further analysis of this very rich collection is hardly necessary. The author gives a biographical sketch of each personality and a brief appreciation of his work, then quotes very fully the essential items. Thus seven large pages are given to MARIA MERIAN, eight to VALLISNIERI, thirty-three to RÉAUMUR, etc. It is not for me to appreciate the author's selection. It must be very difficult to select the most important items, e.g., in the voluminous works of a RÉAUMUR. How wise the author's choice was, only an entomologist could say, and it is probable that no two entomologists would agree, for each has his own pets in the insect world. At any rate, enough information is given about each master to give the reader a feeling of familiarity, and possibly the wholesome desire of drinking more deeply in the original sources.

The volume is splendidly illustrated by means of portraits, facsimiles, etc. I was especially pleased to see good reproductions of insect miniatures taken from Arabic and Latin manuscripts.

The author is the head of the zoological section of the Institute of Natural History attached to the Hebrew University in Jerusalem. Sometime ago in the course of my review of WHEELER's edition of RÉAUMUR's treatise on ants (*Isis*, 9, 445-47), I asked « When shall we be given a good history of entomology? » I did not know then that thanks to the devotion of a Palestinian scholar my wish was already being fulfilled. The first volume of his great work was a great and joyous surprise. May the second volume appear promptly and complete fittingly this splendid offering.

GEORGE SARTON.

Edmund O. von Lippmann. — ***Geschichte des Zuckers seit den ältesten Zeiten bis zum Beginn der Rübenzucker-Fabrikation.*** Ein Beitrag zur Kulturgeschichte. Zweite Auflage mit einem Titelbild und einer Landkarte. IX + 824 p. Berlin, SPRINGER, 1929 (Rm. 66 unbound, Rm. 68.60, bound).

The first edition of this book, appeared in 1890. (Leipzig, M. HESSE; XIII + 474 p.). The author has been engaged in the sugar industry since 1879, and became in 1889 the director of the Zuckerraffinerie Halle in Halle a.d. S. His leisure hours have been given to the history of chemistry, and their fruitfulness may be appreciated by considering the many valuable papers and better still, the series of fundamental works to which they gave birth. A bibliography of them, from 1886 to 1926—18 works and 159 papers—will be found in the Festgabe, edited by JULIUS RUSKA in 1927 to celebrate the author's seventieth anniversary (*Isis*, 10, 268). It will suffice here to recall the main historical works : *Abhandlungen und Vorträge zur Geschichte der Naturwissenschaften* (Leipzig, 1906-13). *Entstehung und Ausbreitung der Alchemie* (Berlin, 1919; *Isis*, 3, 302-05). *Zeittafeln zur Geschichte der organischen Chemie* (Berlin, 1921 ; *Isis*, 4, 548). *Beiträge zur Geschichte der Naturwissenschaften* (Berlin, 1923; *Isis*, 6, 116-19). *Geschichte der Rübe als Kulturpflanze* (Berlin, 1925; *Isis*, 8, 379-80).

The first edition of Dr. v. LIPPMANN's *Geschichte des Zuckers* was a pioneer work, and it remained the standard treatise on the subject in every one of its manifold ramifications. However the author was slowly and steadily preparing a new edition, and to underline its value it will be enough to remark that during the forty years which have elapsed since the publication of the first edition no single item relative to the subject is likely to have been overlooked by him. We may be sure that every piece of information concerning sugar in any possible way was patiently and carefully collected for the new edition.

This history is far more comprehensive than its title would suggest. Its extent will be best explained by using the author's own words.

« Die Geschichte keiner der ' weltwirtschaftlichen ' Waren greift so tief in die der mannigfaltigsten Gebiete ein wie jene des Zuckers : Natur, Anbau und Veredlung seiner Stammpflanze, des Zuckerrohres, verknüpfen ihn mit Botanik, Landwirtschaft und Pflanzenzüchtung; frühzeitig wird er zum Gegenstand der Medizin und Pharmazie, des häuslichen und Luxusverbrauches, der Steuer-, Zoll- und Ausnahmegesetzgebung, der monopolistischen und prohibitiven Bestrebungen; er beeinflusst Sitten, Gebräuche und Lebensgewohnheiten aller Art, belebt Handel, Verkehr und Schiffahrt, fördert in durchgreifender Weise das gesamte Kolonialwesen, spielt dabei eine entscheidende Rolle betreffs der Ent-

wicklung der Sklaverei und zeitigt hierdurch so bedeutsame Folgen wie die ' Afrikanisierung ' Amerikas; er drängt Technologie, Chemie und andere Zweige der Wissenschaft zur Lösung wichtiger praktischer und theoretischer Aufgaben; er veranlasst Nationalökonomie und Statistik, sich mit ihm zu beschäftigen; er regt Schriftsteller an, begeistert Dichter und dient den Philosophen zur Erörterung schwer fasslicher Lehren. »

The subject is divided as follows: (1) Prehistory of sugar. Honey. (2) Native home of sugar cane, and preparation of crude sugar. (3) Sugar cane and sugar in European antiquity and early mediaeval times. (4) Westward propagation of sugar and discovery of refining. (5) Sugar at the court of the caliphs. (6) Sugar cane and sugar in the western caliphate. (7) Diffusion of sugar toward China and the coasts of the Indian Ocean. (8) Sugar at the times of the Crusades. (9) Sugar consumption in Europe in the fourteenth and fifteenth centuries and its sources of supply. (10) Sugar in the sixteenth century. (11) Sugar manufacture in America in the seventeenth and eighteenth centuries. (12) Sugar consumption in Europe in the seventeenth and eighteenth century. (13) European sugar refining in the seventeenth and eighteenth centuries, and in the beginning of the nineteenth . (14) Sugar in the east since the beginning of the fourteenth century. (15) Substitutes of cane sugar. (16) History of the cost of sugar. (17) Views on the origin and nature of sugar.

An appendix contains a chronological list of places showing when sugar cane was first mentioned for each of them. I copy the beginning of it (without the references):

c. 327 B.C.	Beyond the Ganges	c. 850	Andaman & Nicobar I.
c. 200 B.C.	China	c. 900	Tyre, Tripolis, etc.
c. 1 A.D.	East India (Java)	c. 900	Tabaristan, Balkh
c. 400	Kashmir, Tibet	c. 1100	Zanzibar
c. 450	Jundishapur	c. 1100	Morea
c. 600	Ceylon	c. 1200	Majorca
after 643	Egypt	c. 1250	Formosa
c. 680	Syria (Damascus)	1420	Madeira
c. 700	Cyprus	c. 1480	Canaries, St. Thomas
c. 700	Cambodia	1493, 1514	Santo Domingo
c. 700	Socotora	c. 1520	Mexico
c. 709	Morocco	1524	Kabul, Tiflis, Oxus
c. 714	Spain	1532	Brazil
c. 750	Provence	c. 1533	Peru
c. 818	Crete	1549	Provence
after 827	Sicily	c. 1580	New Spain
c. 850	Madagascar	1580	Paraguay.

The primitive area of distribution of the sugar cane : India, Persia, Mesopotamia, and the Near East, is shown on a map.

Dr. VON LIPPMANN might be compared to a bee transforming its food into sugar; the intellectual pabulum of half a century he has changed into this History of sugar, which will remain an eternal witness of his scholarship and devotion. GEORGE SARTON.

Bronislaw Malinowski. — ***The sexual life of savages in north-western Melanesia.*** An ethnographic account of courtship, marriage and family life among the natives of the Trobriand Islands, British New Guinea. With a preface by HAVELOCK ELLIS. 2 vols. XXVIII + 603 p., 96 pl. New York, HORACE LIVERIGHT, 1929 (ten dollars).

The most interesting chapter of this very honest and remarkable study is the seventh (out of fourteen) dealing with Procreation and pregnancy (vol. 1, 164-210). I believe the author has completely established the fact that according to Trobriand lore the child's body is built exclusively by the mother, the father contributing nothing. After surveying their anatomical and physiological knowledge, he explains their views on pregnancy. There is in every man or woman a spirit (***baloma***); after death it moves to Tuma (the Island of the Dead), but later reappears in a woman's body thus causing her to show after a time the usual symptoms of pregnancy. « That all spirits have ultimately to end their life in Tuma and turn into unborn infants; that every child born in this world has first come into existence (***ibubuli***) in Tuma through the metamorphosis of a spirit; that the only reason and real cause of every birth is spirit activity, are facts known to everybody and firmly believed by all ». The spirit is frequently brought to the prospective mother by a godfather, whom she is able to designate. « The spirit-child is laid by the bringer on the woman's head. Blood from her body rushes there, and on this tide of blood the baby gradually descends until it settles in the womb. The blood helps to build the body of the child —it nourishes it. That is the reason why, when a woman becomes pregnant, her menstruous flow stops. A woman will see that her menstruation has stopped. She will wait one, two, three moons, and then she will know for certain that she is pregnant. »

This combination of physiology and spiritualism is very remarkable indeed. It is seen that it rules the father out completely. Yet the islanders admit that a virgin cannot conceive, « virginity mechanically impedes spirit impregnation. »

The theory of spiritual impregnation, strange as it may seem, is somewhat justified by their experience. Sexual intercourse is exceedingly

free among the young unmarried people, and yet unmarried girls have seldom children, in spite of the fact that contraceptives are unknown.

The social organization is based on that theory. Society is dominated by the matrilineal principle. *Tama* (father) is the mother's husband, he is hardly more important than *kadagu*, the mother's brother. Yet if the father is physiologically inexistent, he is not at all unimportant socially. He is the mother's protector, and the protector and tutor of her children. For an unmarried mother, there is literally no father to the child; for the married mother, her husband is always the father of her children, even if some were born during a prolonged absence.

Considering that marriage is the end of an unusual sexual freedom, it is remarkable that men are very anxious to marry. This is explained not only by economic reasons but also by the fact that a man has no full status in social life until he is married. The purely sociological and economical aspect of MALINOWSKI's study is of great importance, but I cannot do more in this place than to indicate it.

The reader will ask himself even as I did: « If these people do not admit physiological paternity, how do they account for the resemblance of children to their father, or their father's kin? » One would assume a priori that under the influence of their peculiar theory of procreation, they would deny the possibility of such resemblance or consider it purely accidental. On the contrary they consider it natural, and the resemblance to the mother, unnatural! « Not only is it a household dogma, so to speak, that a child never resembles its mother, or any of its brothers and sisters, or any of its maternal kinsmen, but it is extremely bad form and a great offence to hint at any such similarity. To resemble one's father, on the other hand, is the natural, right, and proper thing for a man or woman to do »... « I found that everyone in the Trobriands will, in the teeth of all the evidence, stoutly deny that similarity can exist between matrilineal kinsmen. A Trobriander is simply irritated and insulted if striking instances are pointed out to him. » This might be taken by L. LEVY-BRUHL as an admirable proof of primitive lack of logic (*Isis*, 12, 343-47), but the conclusion would not be entirely warranted. The spiritual theory of procreation, however odd, is not inconsistent.

Students of magic will find excellent material in these two volumes, for example, the pregnancy rites, beauty and love magic, division of magic between the sexes (vol. 1, p. 43). « Man and woman have each their own sorcery, carried on by means of different rites and formulae, acting in a different manner on the victim's body and surrounded by an altogether different atmosphere of belief. Male sorcery is much more concrete, and its methods can be stated clearly, almost as a rational

system. The sorcerer's supernatural equipment is restricted to his power of vanishing at will, of emitting a shining glow from his person, and of having accomplices among the nocturnal birds. Extremely poor means of supernatural action if we compare them with the achievements of a witch ! »... « The strong tribal position of women is also buttressed by their right to exercise magic—that toughest and least destructible substance of belief... » « In all this there is a balance between the influence of male and female, the man wields the power while the woman determines its distribution. »

The author of this work is professor of anthropology in the University of London. He was born at Cracow, Poland, in 1884. When ill health interrupted his studies at the University of Cracow, he went away for three years of travel, mostly along the shores of the Mediterranean, where he wandered through the Moorish provinces of Africa, Asia Minor, and through Turkey. He lived for nearly two years in Spain and the Canaries; and then returned to his studies. In Germany he studied psychology under WUNDT and the social sciences under LAMPRECHT. After that he worked at the London School of Economics. Before the outbreak of the World War, he went with the British Association to Australia. When war was declared, he was made prisoner. During this period he made three trips to New Guinea. There he pitched his tent on a coral beach, lived, ate, and talked the language of the New Guinea cannibals. This intimacy of contact gave him unique opportunities. Out of it grew his many works, including that famous epic of a Melanesian trading expedition, *The Argonauts of the Pacific*, *Sex and Repression in Savage Society*, *Myth in Primitive Psychology*, and the present volumes which promise to remain a standard contribution to the subject.

GEORGE SARTON.

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